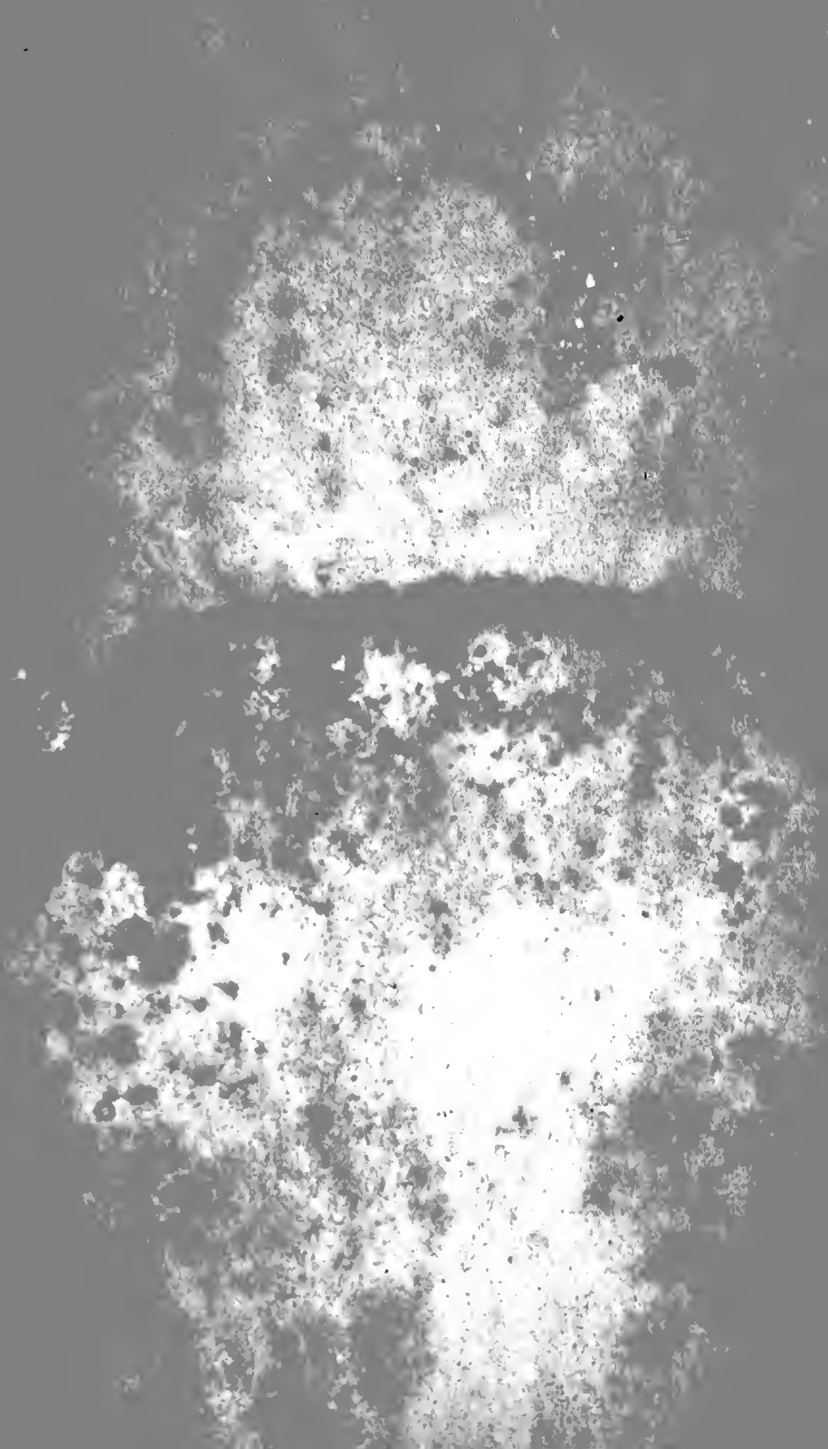

Fig. 1.

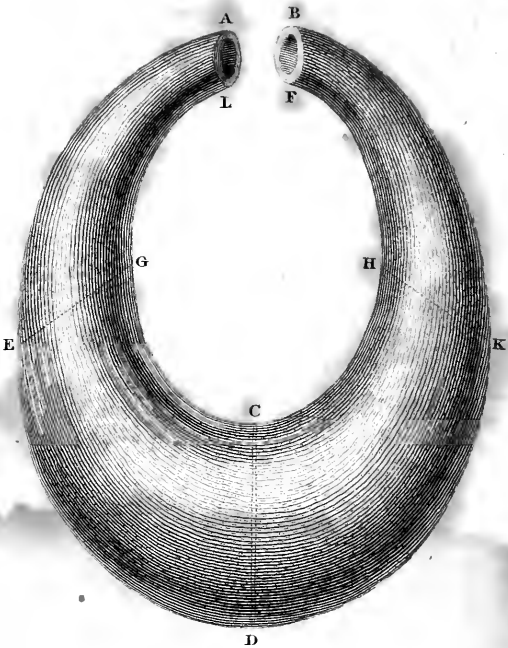

Fig. 2.

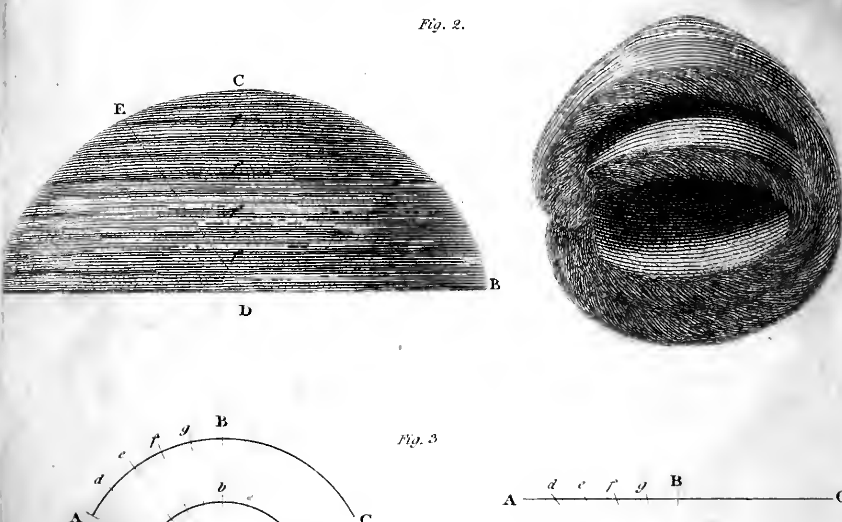

Fig. 3

Thos Smith sculp.

Engraved for Dr Carson's Inquiry.

G. C. Shattuck

AN

INQUIRY

INTO THE CAUSES

OF THE

Motion of the Blood;

WITH AN

APPENDIX,

IN WHICH

THE PROCESS OF RESPIRATION AND ITS CONNEXION WITH THE CIRCULATION OF THE BLOOD ARE ATTEMPTED TO BE ELUCIDATED.

BY JAMES CARSON, M. D.

PHYSICIAN TO THE WORKHOUSE, THE FEVER HOSPITAL, AND TO THE ASYLUM FOR THE PAUPER LUNATICS AT LIVERPOOL; AND IN CHARGE OF THE MILITARY HOSPITAL, AT THAT PLACE.

Although the circulation of the blood has been almost universally acknowledged for above a century past, and much has been written in order to explain that doctrine, yet there are several things relating to it which have not been, hitherto, accounted for in so satisfactory a manner, as to render any further inquiry into them superfluous.

Whytt on the Motion of the Fluids.

Liverpool,

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1815.

TO

SIR JAMES MACGRIGOR, KNT. M. D.

DIRECTOR GENERAL

Of the Medical Department of the Army;

AND TO

CHARLES KER, M. D.

AND

WILLIAM FRANKLIN, M. D.

PRINCIPAL INSPECTORS OF HOSPITALS;

CONSTITUTING THE ARMY MEDICAL BOARD.

GENTLEMEN,

THE following volume professes to treat of those functions, on the knowledge of which the practice of medicine and surgery is unquestionably in a great degree founded. If, therefore, the execution of the work should in any respect correspond with the importance of the subject,

there would be a peculiar propriety in addressing it to those Gentlemen who have selected, arranged, and conducted the medical department of the British Armies, during the period in the history of that department, most distinguished for the science, the zeal, and the industry of its members; for the magnitude and importance of its duties; for the confidence and attachment of the brave objects of its care; and for the fervour and unanimity of applause by which it has been rewarded by a discerning nation.

The liberality, gratifying approbation, and flattering confidence with which my professional services have for a long period been honoured by your board, have filled my mind with respect and gratitude, and rendered the opportunity,

which now offers itself, of making a public acknowledgment of your favours, highly acceptable to,

Gentlemen,

Your most obedient,

And most humble servant,

JAMES CARSON.

Liverpool, 11*th Nov.* 1815.

AN INQUIRY

INTO THE

CAUSES OF THE MOTION OF THE BLOOD.

—o—

Two Centuries have now nearly elapsed since the Circulation of the Blood was first taught by Dr. Harvey. This theory has long surmounted the opposition against which it had for a period to contend from inveterate prejudices and jealous ambition; is universally acknowledged to have been fully proved by its author; and is deservedly ranked among those discoveries which are supposed to confer the greatest honour upon human nature.

Although, however, the course which the blood pursues in its march through the animal frame has been ascertained to the conviction of every inquirer, differences of opinion are still

found to prevail respecting the causes by which it is moved in that course. Considering the immense progress which the mechanical sciences have made since the days of Dr. Harvey, and the attention which so important a subject must have at all times engaged, it might at first view have been expected that the causes of the motion of a fluid, of which the path had been for ages so fully known, must long ago have been ascertained with that mathematical precision which is incompatible with difference of opinion. When, however, it is considered that the machinery employed in the motion of the blood is carefully concealed from the view; that every attempt to unveil the hidden springs of this machinery necessarily destroys it; and that the delicate and exquisitely appropriate movements of living organs cannot be imitated by any artificial process, the surprise, excited by the subject being still involved in obscurity, will soon be removed.

With respect to the sentiments which are at present entertained concerning the causes of the motion of the blood, physiologists may be divided into two classes. The first maintain that the blood is transmitted from the heart to the extreme vessels, and from the extreme vessels back to the

heart again, by the projectile power of this organ aided by the vibrations of the arteries. The other class contend that the force impressed upon the blood by the heart and arteries is expended, or nearly so, at the terminations of the arterial system; and that the motion of this fluid while in the veins arises from causes distinct from those by which it had been produced in the arteries.

The more recent physiologists have generally arranged themselves in the latter class; but though they have succeeded in opposing insuperable objections against the supposition of the blood in the veins being solely returned by the joint action of the heart and arteries, conveyed to it through the extreme vessels, or in the language of the schools, by a *vis a tergo;* they have hitherto failed to explain its motion in this part of the sanguiferous circle upon any other hypothesis.

At an early period of my medical studies, an accidental occurrence fixed my attention more particularly on the circulation of the blood. In the course of that investigation, I was struck with the wide diversity, and even opposition, in the opinions entertained by writers respecting

the causes of the motion of the blood, especially in the veins; the inadequacy of these causes in many instances to the effects ascribed to them; and the generally unsatisfactory and defective explication of the phenomena deducible from any hypothesis.

At length I thought I perceived, obscurely marked on the motion of the blood, the vestiges of powers, the influence of which had been unnoticed, little valued or erroneously estimated by physiologists; and I ventured to explain my sentiments respecting their importance in the circulation, in the Inaugural Dissertation which I published at Edinburgh, in the year 1799.

Since that period, I have frequently, after intervals, returned to the consideration of the subject; and, though I have been able to discover abundant errors and great deficiencies in the Dissertation; I have been more and more convinced, by every succeeding reflection, that the sources of the motion of the blood, while in the veins, have not been detected; and that an important share of this motion belongs to the powers to which I have alluded.

That the foundations upon which I have endeavoured to establish doctrines, sometimes altogether new, and frequently different from those generally received, might be as solid as possible, I had intended to have given them at every point the support which I conceived might be derived from fresh experiments. As such experiments must often have had for their subject the living machine, and could only be relied upon when conducted with great skill and delicacy, and must, therefore, have required uninterrupted leisure, and such a favourable assemblage of other circumstances as it was not likely would be soon possessed; I have been obliged to remain satisfied in a great measure with the data derived from the facts which are open to common observation, or which have been detected by the experiments and reasonings of preceding inquirers. Nor will there, I trust, be much reason to lament the disappointment. It is, perhaps to a censurable degree, the fashion of the philosophers of the present day, to aspire after distinction by the number and variety of new experiments, rather than by weighing well the tendency and value of those which have been already made.

It was intended at first to limit the inquiry to the motion of the blood while it flowed in the veins; but it was found that all the parts of the circulation were so connected, that they could not with propriety be separately considered.— I have been therefore induced to take a general survey of the causes of the motion of the blood; and, it will be found that in those parts of the sanguiferous circle, in which the motion of this fluid has been supposed to be most satisfactorily accounted for, views have occasionally arisen, at least different from those which have been presented by preceding writers.

The following pages will contain,—

I. An enumeration and estimate of the causes which have been supposed to promote the circulation of the blood.

II. The developement of other causes which appear to contribute to the motion of the blood; and which, added to the preceding, will, it is presumed, be found adequate to the effect.

III. An explication of phenomena.

PART I.

—o—

BEFORE we proceed to enumerate the agents which have been supposed to contribute to the motion of the blood or to attempt to estimate their share in the operation, it seems proper to describe the organs in which this fluid is contained, and the parts more intimately connected with them; but the account to be given of these in this place need only be concise and general, as it may be required to describe them more minutely afterwards, when the uses of each shall come to be investigated.

The heart, the arteries and the veins, the organs in which the blood circulates, demand our first attention.

The general appearance of the heart is familiar to every one, and is nearly the same in all animals. It is usually said to resemble a cone, with an obtuse apex, a rounded or oval base, and at the sides more or less swelling or convex. Ex-

ternally the heart is every where smooth, polished, and free, except at the base where two rough, notched and ear-like appendages, hence denominated auricles are attached, and where a number of vascular tubes penetrate it and connect it to the surrounding parts. Internally it is hollow and divided into four chambers, of which two, denominated ventricles, are contained in the body of the heart, and the other two in the appendages just mentioned are termed auricles. It is, besides, divided into two parts between which there is no direct communication by a septum or middle partition, which has on each side of it one ventricle and one auricle; those upon one side receiving the epithet of right, and those on the other of left. The right auricle opens into the large venous trunk, called the vena cava; and communicates by another passage with the right ventricle, which, besides the outlet common to it and the auricle, has a communication with the great pulmonary artery. The auricle on the other side of the partition, communicates by distinct openings with the four pulmonary veins, and also by another passage with its corresponding ventricle; which, besides this mutual outlet, is penetrated by the large artery called the aorta.

The ventricle and auricle therefore on each side of the middle partition, besides their mutual communication, have outlets into the trunks of the blood vessels; the auricles communicating with the roots of the veins, and the ventricles with those of the arteries.

The structure of the heart is muscular and tendinous. The walls or substance inclosing the different cavities vary greatly in thickness. The walls of the ventricles are much thicker and more substantial than those of the auricles, and those of the left ventricle than those of the right. The thickness of the walls of the respective ventricles is pretty nearly uniform; but the walls of the auricles are extremely unequal, being composed of muscular and tendinous ridges, which especially project inwards, and which are connected only by a thin, transparent, expansive membrane.

Valves are stationed at all the openings into the ventricles. Those at the passages between the ventricles and auricles, usually called the auricular passages, are framed to favour the free current of a fluid from the auricles into the ventricles, but to obstruct its return from the ventricles into the auricles. Those at the roots of the

arteries are calculated to prevent the return of any fluid from the arteries into the heart, without impeding its free passage from the heart into the arteries. No valves are stationed at the openings by which the auricles communicate with the large venous trunks. The chambers of the heart, in a man full grown, are each supposed to contain about two ounces and a half of fluid.

The arteries arise in two trunks from the ventricles, one from each. They are hollow cylinders, and at a short distance from their origin branch into other vessels of the same description. Every branch becomes the stem of a smaller order of branches, and this process continues till the ultimate ramifications become so minute that their bore will scarcely admit a single hair, and so extended and numerous as to penetrate almost every particle of the body

The rule observed with respect to the dimensions of these cylinders is this:—the bore of the stem is always greater than that of any of the branches, but less than the bore of all the branches united. Hence the arteries have been properly said to constitute a cone of which the apex is at the heart, and the extended base is measured by

the united capacities of the extreme capillaries. The bore of the trunk of the aorta compared with that of the branches united into which it has ultimately divided, has been attempted to be determined, but the conclusions to which these attempts have led, have been often widely different. According to Keil, the bore of the aorta is to that of the extreme vessels united as 1 to 44,500.—According to Helvetius and Sylva, as 1 to 700: as 90,000 to 118,490, according to the calculations of Senac. Martin maintains that the diameter of a trunk or stem is equal to the cube root of the diameters of its immediate branches united. Calculators have not been able to arrive at any certain conclusions on this subject. Some imperfect notion may be formed of the difference of the capacity of the aorta and that of its ultimate ramifications united, by considering the different velocities of the blood in the aorta and in the extreme vessels; as the square of the diameter of the aorta will be to that of the diameter of all the last ramifications in the inverse ratio of the velocities of the blood at these parts. From the observations that have been made respecting the motion of the blood in the extreme vessels of transparent parts, it is evident that the capacity of all the ultimate branches must in a vast degree exceed that of the arterial trunk.

The arteries are of a strong and firm structure. They are composed of different coats each of which seems to confer upon them peculiar properties.—These coats are generally reckoned to be four, but anatomists are not altogether agreed respecting their number. The coats most particularly deserving of notice in this place, are the muscular and tendinous, and these are universally allowed. The muscular fibres are very numerous and generally pursue a circular or spiral course round the cylinder, though they are frequently found to run in a direction nearly parallel to its axis. The muscular coat confers upon the arteries the property of irritability; as the tendinous structure gives them that of elasticity, which they unquestionably possess in a considerable degree; for, when taken out of the body and empty, their transverse section forms a complete circle.

The veins, as has been stated, arise in different trunks from the auricles. They are also hollow cylinders, and with respect to their course, form, and laws of ramification, agree with the arteries already described. They are generally larger and more numerous than the concomitant arteries; so that the capacity of all the veins is supposed to be to that of all the arteries as 2 to 1 or 3 to 2.

Their structure is very different from that of the arteries. They have no muscular coat, at least the muscular fibres are few; they therefore can scarcely be said to be irritable. They are much thinner than the arteries of the same size; are very dilatable; but possess little elasticity; their transverse section does not form a circle. But though they are thin and readily yield to a slight distending force they are not easily ruptured; indeed they are found to sustain, before they break, a greater force than the arteries which accompany them.

The internal surface of the veins differs in one remarkable particular from that of the arteries. In the arteries it is uniformly smooth and equal. In the veins there are frequent projections formed by the foldings of the inner coat. These projections are circular segments, and are formed in pairs or triplets at a place. They are very numerous in the veins that run nearest the surface of the body, but are not found in many of the veins of the internal viscera. They are supposed to serve the purpose of valves; obstructing the current of a fluid from the heart to the extremities, without impeding its course in the opposite direction.

The veins which arise from one stem frequently communicate with each other and with those of other stems by intervening vessels, which are called by anatomists anastomosing vessels. These anastomoses are found in all parts of the venous system, but are particularly numerous among the smaller vessels. We shall have occasion to treat of these more particularly in a subsequent part of this inquiry.

The mode of communication between the ends of the arteries and the ends of the veins has been long a subject of dispute among anatomists, and is still in some degree undetermined. It was long warmly maintained that the extreme arteries and extreme veins coumunicated through the intervention of cells. Hence the *corpora nodosa* of authors. It is certain that in some parts of the body the arteries pour their contents into cells from which it is taken up by veins, as in the corpora cavernosa penis, in the mammæ, in the uterus, &c. This structure is generally considered however to be peculiar to particular parts, and by no means an example of the manner in which the arteries always, or even generally, communicate with the venous capillaries. It is now usually supposed that the ultimate arteries enter the ends

of the veins immediately but, in such an oblique manner that the side of the penetrated vein forms a valve, opposing the course of a fluid from the vein into the artery.

The arteries, besides their communication with the extreme veins, have other terminations.—They often terminate in glands by which peculiar fluids are secreted. They become exhalent vessels opening on surfaces, and pour out a transparent fluid for the lubrication of parts. The veins also, there can be little question, often terminate on surfaces and act as absorbents.

An artery is generally accompanied by one or two veins, which with a nerve are inclosed in a common sheath, and are embedded in a cellular substance.

The heart is inclosed in a membraneous bag of a very strong and firm texture. This bag is called the pericardium. It is no where attached to the heart, which is every where free; and is only connected to it by the roots of the large vessels which penetrate it soon after they have issued from the heart, and to which in their passage it communicates a covering. The pericardium is

firmly bound to the tendon of the diaphragm on which it rests. This envelope is found in all animals.

Particular appellations are given to different divisions of the circulating vessels. The aorta and all the vessels of which it is the original trunk are called the system of the great aorta or aortic system. The vena cava and its ramifications are termed the system of the vena cava or simply cava. In like manner the pulmonary vessels are divided into the system of the pulmonary artery and system of the pulmonary veins.

In the brief description about to be given of the march of the blood through the body, every term is purposely avoided which might convey theoretical notions respecting the action of any of the circulating organs.

Let it be supposed that the blood has reached some particular stage in the circulation, and let that stage be the ends of the arteries of the aortic system. Having been poured in infinitely small streams from the innumerable ends of the capillary arteries, the blood passes into the ends of the capillary veins which are at least equally

numerous and minute. In these vessels, like rivulets descending from their sources, the small streams frequently unite and form larger, till, after a long succession of confluences, the blood reaches the right auricle of the heart, contained in a single channel formed by the meeting of the two branches of the vena cava. The right auricle as soon as it has been filled from the cava is successively emptied into the right ventricle. The blood therefore, does not now enjoy the continued smooth course with which it had flowed in the veins, but is transmitted in divided portions with regular succession, into the right ventricle, which successively, as it receives the contents of the auricle, conveys them to the pulmonary artery. In following the ramifications of this artery, the blood is distributed through the whole substance of the lungs, subject throughout to alternate accelerations and retardations. From the ultimate terminations of the pulmonary artery it passes into the ends of the veins of the same system, where it resumes the equal continued flow which it had enjoyed in the system of the vena cava; and, by the frequent junction of the smaller veins, is conveyed into larger and less numerous channels, till it reaches the left auricle, issuing from the mouths of the four pulmonary veins. From

the left auricle it is forwarded to the left ventricle, and from this ventricle into the great aorta, in the same manner in which it had passed through the right side of the heart; and is distributed in a regularly retarded and accelerated motion in the extensive ramifications of this system, through all parts of the body, except the lungs, till it reaches the ends of the arteries, the stage at which it had, according to this description, commenced, to repeat again and again the same unvaried round, often undisturbed by one alarming pause, through the whole course of a long life.

Such is the course which the blood describes according to the Harveian theory; a theory which must form the foundation on which the true principles of the science of medicine, if it shall ever attain the rank of a science, must for ever rest.— It is foreign from the purpose of this inquiry to examine the proofs by which this theory has been established. It is sufficient to state that they are universally admitted to be conclusive.

It appears then that the two sides of the heart, though they may have no direct communication, have yet an intercourse through the intervention of the blood vessels. The extremities of the ar-

teries, arising from the right ventricle, do not communicate with the extremities of the veins which branch from the corresponding auricle, but with the extremities of the pulmonary veins. In like manner, all the ramifications of the aortic system pour their contents into the veins belonging to the system of the vena cava. The blood therefore, before it returns to any given stage in the circulation, washes the walls of the heart twice; passing through the chambers of one side of this organ, on its advance; and those of the other side, on its return. In completing the whole of its course the blood describes the figure 8; the heart being placed at the point at which the two circles touch or their lines decussate. It evidently performs two circulations; the one through the lungs, in consequence of the shorter circuit, is termed the less circulation; which was known and described with considerable exactness by the physicians of the Italian school, long before the days of Dr. Harvey; and the other, as it embraces the extreme parts of the body, the greater circulation.

The human heart, therefore, is a double organ, having two chambers for the reception and two for the discharge of blood. But this two-fold property is by no means necessary for the performance of its functions; nor is it to be found

in all animals. Those animals commonly termed the cold blooded, as fishes, reptiles, insects, &c. have only a single heart consisting of one auricle and one ventricle, one chamber for the reception and one for the discharge of blood; with one system of vessels belonging to each, one system of veins and one system of arteries. This view of the circulation is more simple but not less perfect.— Take again the termination of the arteries as the stage at which the blood commences its march. It passes from the arterial into the venous system, through the terminations of each; is conveyed through the converging venous system in a placid continuous flow to the auricle; which, at intervals as it is filled, empties itself into the ventricle. By the ventricle, in like manner, the blood is transmitted into the root of the arterial system, in pursuing the diverging branches of which it is brought to the stage at which its motion was supposed to have commenced.

In the following pages it may be necessary, to avoid circumlocution, and for the sake of perspicuity, to regard the human heart as a single organ, consisting of one ventricle and one auricle, with one system of veins, and one system of arteries; in which case, the larger circulation will be always understood.

We are now prepared to consider the action of the circulating organs, and its influence in promoting the movement of the fluid they contain.

It may be necessary in this place to premise that any investigation of the causes of muscular motion or of that property of muscles, in consequence of which, upon the application of certain stimulating substances, or in obedience to the will, or through sympathy with other muscles, they are contracted, is not aspired at in this inquiry. We are therefore relieved from the consideration of those opinions of which all ages have been so fruitful respecting the causes of the action of the heart; and which, as they are purely hypothetical, may fairly be presumed to have been in every instance erroneous.

As further, the effects of the actions of the circulating organs upon the motion of the blood could not have been ascertained, with any degree of correctness, before the direction of that motion was known, it seems unnecessary to allude to any opinions that were entertained respecting these actions previous to the discovery of the circulation.

It had been observed, from a very early period, that different parts of the heart were alternately contracted and dilated. It has been found that the body of the heart, or that part of it in which the ventricles are contained, is all contracted or dilated at the same instant; that the two auricles or appendages are also synchronous in their movements; but that the dilatation of the auricles corresponds in point of time with the contraction of the ventricles, and vice versa. The similar conditions, therefore, of the two ventricles are synchronous, but alternating with those of the auricles which are also synchronous. These conditions of the heart are expressed in the language of anatomists. The state of expansion or dilatation is called their diastole, while the opposite state of contraction is termed their systole.

Let the description to be given of the movements of the heart commence with some supposed state of that organ, and let that state be the diastole or state of greatest distension of the ventricles, and, of course, the systole or state of utmost contraction of the auricles. In this situation the ventricles are distended with blood while the auricles are empty or nearly so. In consequence of a stimulus derived from the blood and from the distension of the muscular fibres,

the ventricles are roused into action; they contract forcibly upon the blood with which they are filled, and propel it through whatever outlets may occur. These outlets we have seen are two in each; one into the great artery and another into the corresponding auricle. The blood however is prevented from flowing into the auricle, which is at this moment empty, by valves; which are stationed at the opening into it, and which, as was observed, are so formed as to prevent the blood flowing from the ventricles into the auricles; but there is nothing to impede its course into the artery; it therefore flows with great rapidity through this vessel. The ventricle being now in the systole, the auricle becomes dilated. Whence is derived the blood by which the auricle is to be filled? The auricle is penetrated by two openings, one common to it with the corresponding ventricle; the other proceeds from the large vein. It can receive no blood from the former opening in consequence of obstructing valves.— It must therefore be filled from the vein. The auricle being in its turn dilated and filled, contracts upon the contained blood driving it into the ventricle which is, consequently, again distended and filled. The blood, with which the ventricle is filled afresh, must necessarily be derived from

the auricle; for, at the only other entrance into it, that from the great artery, valves are stationed which prevent the return of any fluid from that vessel into the ventricle. The actions of the ventricles are successively repeated in the same manner and with the same consequences that have been described.

At one part, this machinery appears to be imperfect. No valves are to be found at the passages between the veins and the auricles. It was to have been supposed, that during the contraction of the auricle the blood would have flowed as readily into the vein as into the ventricle; and, as the latter was to have been dilated by the force of the blood from the auricle, that it would have found the course into the vein more open and less resisted. But it is supposed that the force of the blood on its return to the heart is so great as to form a buttress to the blood in the auricle, and to direct all the power of this chamber against the ventricle.

The systole of the heart was easily admitted on all hands to have been produced by the peculiar property of muscles, that of contracting their fibres upon the application of stimuli. How are the cavities again enlarged? The muscles of the

heart being hollow has no antagonist by the action, of which as is the case with the generality of other muscles, the contracted fibres could be again distended. The simple relaxation of its fibres could not dilate the cavity but only place those fibres in the situation most favourable to be dilated by other agents. It was contended by Dr. Harvey and his followers, and the opinion still prevails, that the auricle is dilated by the force of the blood returning to it from the veins, and that the ventricle is dilated by the force of the blood impelled against the internal surface of its walls, by the contracting auricle.

Whence does the blood at the root of the vena cava receive such a quantity of motion, as is not only sufficient to carry it with great velocity along the channel of the vein, but, in addition, to impower it to resist the contraction of the auricle, and to distend the cavity of that chamber? Dr. Harvey, and many of his followers, contended that this power is derived from the left ventricle of the heart, extending through the aortic and venous systems back to the right auricle. Hence arose a question that for a long time occupied the ablest mathematicians of that brilliant period, "What is the power of the heart?" Soon after the

the theory of the circulation had been promulgated and generally admitted, the fluxionary calculus was invented by Newton and Leibnitz independently of each other; and, in consequence, the empire of science seemed to be shared between England and Germany. This mighty engine of discovery, which from its vast success seemed almost omnipotent and fitted to level all nature before it, by reducing every thing to its original principles, was applied, by many of those who could most powerfully and skilfully direct its energies, to the ascertaining the power of the heart. But the amazing diversity in the results, to which the candidates for the solution of this question were brought, proves, in this instance, the futility of their labours. While the calculations of Borelli advanced the power of the heart so enormously as to render it equal to the weight of 180,000 pounds; those of the equally learned and ingenious Keil reduced it to a force measurable by the weight of 8 and in another instance 5 ounces. Different weights as measuring the power of the heart, between those two extremes, have been contended for by Jurin, Hales, Robinson, and others.—The problem, however, it is to be lamented, still remains unresolved; and physiologists are at this day obliged to confess,

that there exist no rules by which the power of this organ can be estimated.

But however unlimited the power of the heart may be supposed, it will be allowed that the effects of this power, must, like those of all other agents, be liable to expenditure; and that the motion, communicated by it to the blood, must be again surrendered by that fluid, according to laws applicable to it in the circumstances in which it may be placed.

It is plain that, in any given time, the same quantity of blood is returned to the heart by the veins that is discharged from it into the arteries. It follows also from this that, in the same times, equal quantities are transmitted through any sections of the aorta and pulmonary artery before their bifurcation, or through any sections of their branches taken together, at the same relative distances from the heart, at whatever distances those sections be taken, as from the heart itself. That the adequate supply may be provided in time at the heart, it follows necessarily that equal quantities must, in a given time, pass through any sections of all the branches of the vena cava and pulmonary veins, taken relatively at proportional

distances, that passes through the sections of the trunks of the vena cava and pulmonary veins before their bifurcation.

To render this argument more perspicuous, let us for a moment return to the supposition of a single circulation as is to be found in the more imperfect animals. And let it be further supposed that the arterial and venous systems form only a single canal; that this canal is of the same length with the arterial and venous systems united, but that its capacity, at the commencement and termination, or at the two ends, is equal to that of the trunk of the great artery and great vein; that, at any intermediate section, it varies according to the joint capacities of the branches, at corresponding sections of the arterial and venous systems. From the description which has been given of the arterial and venous vessels, it is evident that this canal will be constituted by two cones joined to each other at their bases; that these cones will be of equal length, but varying in width or capacity; that representing the venous being larger than that representing the arterial vessels.

Let such a canal be supposed to be represented by the figure A C B D, (fig. 1.) As this canal is

supposed to be every where filled with blood; and, since as much is supposed to be discharged from one end of it as has, in a given time, entered at the other; it must follow, that the same quantity of fluid will pass in the same time through all the different sections of this canal. In any given time as much will pass through the section EG as enters the canal at AL. The same may be said of CD, HK, or BF, or wherever any transverse section may be supposed to be taken.

The velocity of the blood must vary in different parts of this canal; it will be greatest at the commencement and termination, and least at the middle. It will be greatest at AL and BF and least at CD. The velocity at the different parts will be in an inverse proportion to the areas of the tranverse sections at those parts.

The quantities of motion possessed by the blood in different portions of this canal will be, in given times, as the length of those portions; and in equal portions the quantities of motion must be equal. Let AE, EC, CH, and HB be supposed to measure equal lengths of the sanguiferous canal. The quantity of motion which the

blood, in the same time, possesses in these different portions will be equal.

Upon the supposition, that the blood is circulated by the impelling power of the ventricles alone, this effect must be produced in one of two ways. Either 1, At every contraction of the ventricle, the heart must charge every individual particle of blood issuing from it at that time with such a momentum as shall be necessary to carry it to the end of its course with the momentum which it is known to possess there. Or, 2, The sanguiferous canal having been already filled with blood and an additional quantity having been forcibly thrown into one end of it by the heart and prevented from returning by the semilunar valves, an equal quantity is in consequence of the action of the heart necessarily displaced from the other end of the canal. Let us consider briefly each of these propositions. Let it be supposed that as much force is impressed upon every particular discharge of blood as shall be capable of conveying that blood from one end of the canal to the other, or from the heart through the arteries and veins back to the heart again.—This supposition, so far as the individual blood projected is in question, must from the nature of

fluids in motion, and the concomitant circumstances, at the first view be evidently inadmissible. For the canal, into which the blood is projected by the heart, being already in a great measure filled with blood ; a communication of motion between the particles forcibly discharged and those already in the canal and nearly at that time at rest, must necessarily take place ; and such a resistance be immediately opposed to the former as nearly to destroy the motion they had received from the heart, or even from the collision to drive them partly in a retrograde direction.

But it may be maintained that the effect may be equally produced by transmission of motion from the particles sustaining the original impulse to those against which they are impelled in a more advanced part of the canal ; and that those again communicate a motion to others in a still more advanced situation, and so on, till the blood, which happens at the time to be in the roots of the cava, is urged against the internal walls of the auricle, in consequence of a power transmitted to it by succession from the ventricles of the heart. Suppose that the heart has thrown into the canal AG at AL a quantity of blood so forcibly as to produce in the portion of the canal AG a cer-

tain quantity of motion, in a given time. According to supposition, the motion existing, in the same time, in the next equal portion of the canal has been generated by the impulse sustained from the blood in AG. But from what has been stated, the quantity of motion in ED in the time given is equal to that in AG. Further, the quantity of motion in the next equal portion of the canal CK is equal in equal times to that in ED. But the motion in CK is by supposition all derived from ED. And so of the motion of the blood in every other equal portion of the canal. The heart, therefore, is supposed to produce an effect amounting to that of communicating to a body a certain quantity of motion, and at the same instant the property of retaining, without further aid, that motion undiminished through a long course, in which the moving body is subjected to various powerful resistances. But this, from the established laws of nature, is known to be impossible.

It may be further urged, that the blood already in the vessels at the time that a fresh impulse is communicated to it from the heart, is not at rest but in motion, and that in consequence of the new motion added to that which is already possessed

the blood is impowered to transmit the supposed motion to the blood in the next portion of the canal. But if a quantity of motion belonged to the blood already in the canal, at the moment at which an additional impetus was given to it, by the renewed action of the heart: that motion, after the transmission of a quantity equal to that just received, must be admitted to have remained in whole or in part, or to have been entirely annihilated. If it remained in whole, as much had been transferred to the succeeding pulsation as had been received from the preceding, and no actual aid obtained. If in part, that part must decrease after every pulsation till it is reduced to nothing, and the blood be brought to rest between the impulses: and the state is only for a short time deferred to which it would be immediately reduced by its instantaneous and complete annihilation.

All the motion existing in the whole vascular system during the time intervening between the commencement of one pulsation and the commencement of the next, must be admitted upon the supposition now made to be the effect of a single impulse of the ventricles. We are, therefore, unavoidably brought to the same conclusion; that a motion has been generated of such a de-

scription, as to continue undiminished, in opposition to gravity and many other causes of retardation.

To avoid the mechanical absurdity necessarily involved in this conclusion, Harvey and many of his most able followers maintained, that one wave of blood having become slower is overtaken by another more rapid, and by it impelled forcibly forwards. This, however, is only re-repeating, in other words, the supposition which has been already refuted. Even though the portions of blood, between which the collisions are supposed to be made, were perfectly elastic, no new motion could in that way be generated; and as much motion would be lost by the impelling as had been gained by the impelled body. But as the blood is a tenacious inelastic fluid, it is evident that a great loss of motion must result from such a collision between its particles.

But granting even that the heart were capable, by some unknown mode, of communicating the requisite motion to the blood in the roots of the cava, there is still the further admission necessary, that the vessels be always filled to that degree of distension which would give them the property of rigid undilatable tubes.

2. This suggests the consideration of the other mode by which the blood may be supposed to be circulated in consequence of the impelling power of the heart alone; which is, that the vessels having been already fully distended with blood, and an additional quantity having been thrown into one end of the canal, and prevented from returning out of it by valves; an equal quantity, to afford room for that injected, must necessarily be displaced from the other end of the canal;—out of the roots of the cava into the right auricle.

Though we are not acquainted with any data from which the power of the heart can be calculated, there must exist, nevertheless, certain limits, within which it must reasonably be supposed to be confined. If we consider that the quantity of blood in circulation, is nearly one fifth of the weight of the whole body; that this great mass is spread over an immense surface; that it is, therefore, subjected to great resistances from friction, especially in the small vessels, where each globule is to be rolled over a fixed surface, and often pressed into an oblong shape, before it can pass through canals of which the diameter is less than its own; that the currents, in consequence

of anastomosing branches, are perpetually flowing in opposite directions; and that attraction must powerfully prevail between the blood and the small vessels: when we consider the mass moved, the motion with which it is moved, and the resistance opposed; it is impossible to imagine that this labour could have been performed by the propelling power of the ventricles.—Besides, all this immense force must have been sustained by the root of the large artery which would be constantly strained to a vehement degree, and would at every pulsation incur the danger of rupture.

But admitting, contrary to all probability, that the power of the heart was capable of effecting the motion of the blood in the way supposed; the structure and condition of the vessels in which it flows, evidently prove that it is not circulated by this power alone. Before any blood could be displaced from the end of the canal, in point of action most distant from the moving power, it is necessary to suppose that the vessels have, at every intermediate section, been filled to that degree of distension, which would be occasioned by the weight of a column of blood whose height is equal to the distance between this section and the right auricle of the heart, (taking

the measurement according to the track of the blood) and whose base was equal to the area of the transverse section of the vessel at this distance. The distension which the weight of such a column would produce would be immensely increased by the resistances above enumerated. But the blood vessels do not appear to sustain any considerable degree of lateral pressure. The veins, in particular, vessels, which are thin and eminently dilatable, even in those cases in which gravity acts in opposition to the moving power, are by no means dilated generally to their largest diameter; as may easily be observed by surrounding the leg or arm with a ligature.

Life would, in this case, have been a very insecure tenure. Every considerable loss of blood would have been followed by a permanent stoppage of the circulation. For, unless the vessels were always filled to a proper extent, no blood would be found at the trunk of the cava to dilate and fill the auricle. The heart would be left dry by every profuse hæmorrhage.

Had the blood been circulated by the heart alone, this organ might have been expected to bear, in different animals, some proportion to

their size; but this is not the case, the heart of an ox does not bear nearly the same proportion to the bulk of his body, that the heart of a dog bears to his.

The emptiness of the arteries and the fulness of the veins after death, prove that the blood, which had been in the arterial system must at, or before, the time of death, have been influenced by some power distinct from the heart.

Many other arguments might be advanced in refutation of the doctrine, that the blood is circulated by the heart alone; but it seems unnecessary to notice them; especially as it is now generally allowed, that the heart is powerfully assisted in this operation by the blood vessels and other agents.

It was at all times observed that the arteries, in many parts of the body at least, were characterised by a regularly repeated, sharp stroke, which in common language, was termed the pulse. Various explanations have from an early period been proposed of the causes of this phenomenon. When the circulation of the blood became known, it was generally admitted that this

beating of the arteries was primarily produced by the impulse of the heart; and after the following manner.

The heart, during its contraction, was supposed to throw, with great force, a quantity of blood into the arteries, and to communicate an impulse to the whole blood in the arterial system. The coats of the arteries necessarily have to sustain this impulse; but, being elastic and dilatable, they give way and become enlarged in diameter, as well as lengthened in axis; and, in consequence of the increase of dimensions, thus suddenly obtained, impart a stroke to the bodies that may be in contact with them. But this stroke could not be constantly repeated without an opposite action. There must be a recovering as well as a distending power. This antagonist action was easily deducible from the properties of the arterial structure. The impelling power of the heart having terminated; the arteries, now roused by the stimuli of the fresh blood, by the excitement arising from the over distension of their muscular fibres, and by the elasticity strongly operating, in consequence of the unnatural distension of the elastic substance; in their turn becoming predominant, recoil upon the blood;

and, as rapidly as they had been distended, recover their former diameter and axis. The systole of the arteries having thus been restored; a quantity of blood, as it is an incompressible fluid, equal to that which had been injected into their cavity by the heart, must necessarily have been again displaced. Valves prevent the return of any fluid back into the heart during the recovery of the arterial systole. It must therefore have been discharged by the only other outlets,—the extreme orifices of the capillary arteries into the capillary veins.

The arteries by thus restoring to the blood the force that had been expended in dilating them, were supposed to have contributed greatly to the motion of that fluid through their cavities. But it does not appear that any actual aid could be given to the power that originated the motion by the arteries acting in the manner now stated; for, as was judiciously observed by Baron Haller, the same power, which was required to dilate the coats of all the arteries, would have been sufficient to have transmitted the same quantity of blood through the arteries, if undilatable tubes, in the same time.

Objections, afterwards started, to the explanation just given of the beating of the arteries, suggested at length different views of the action of these vessels and of the manner in which they contributed to the motion of the blood. Weitbrecht, a physician of St. Petersburg, first remarked, that the quantity of blood, thrown into the arteries during the contraction of the ventricle, could not, when divided through the whole arterial system, produce such an extension of these vessels as to enable them to communicate a perceptible impulse. The left ventricle of the heart contains nearly two ounces and a half of blood, and is not entirely emptied during the contraction. But admitting that two ounces of blood are thrown by each contraction of the ventricle into the arterial system, this small portion is evidently insufficient to increase, by its addition, the bulk of the whole mass in a perceptible degree. It has been calculated that the radial and temporal arteries, which are each about three lines in diameter, and in which the beating is very strong, would not be augmented in diameter, by their share of the addition, beyond the twenty-sixth part of a line; an increase of capacity too small, however rapidly made, to communicate any sensible impression.

It might have been added that, from the dilatable property of the arterial trunks, it was not to have been expected that the whole blood in the system could have been affected by the small quantity projected into them by the heart. For the impulse must necessarily have been sustained, in the first place, by the coats of the arteries nearest to the impelling power; and the portion of those vessels which, when dilated, exceeded the capacity of the same portion in a state of contraction, by as much as was equal to the quantity admitted, must alone sustain the shock of the heart. From the dilatable property of the vessels, and the strong resistance opposed by the blood already in the arteries, it is presumable that this portion could not be very extensive.

The pulse having been observed to be of different quickness, at the same time, in different parts of the body, seemed to confirm the opinion, that the pulsations of the arteries were not all the immediate effect of the heart.

From the days in which Weitbrecht flourished, it has generally been admitted by physiologists, that the dilatation of the whole of the arte-

rial system could not be produced by the propelling power of the ventricle. The circulation through the arteries is now generally supposed to be performed in the following manner.

By the contraction of the left ventricle, a quantity of blood is thrown forcibly into the aorta, and an impulse communicated to the blood contained in that vessel. From the known property of fluids in motion, this impulse is necessarily sustained by the sides of the vessels in which it flows; and, as those are elastic, stretches them and enlarges the cavity formed by them. The impetus of the heart, therefore, is expended in dilating through the medium of the blood, a part of the arterial system. But the arteries, being irritable and elastic vessels, now stimulated and stretched beyond their natural condition, recoil upon the blood, by which they were distended; and, by the rapid recovery of their former diameter and axis, restore to it the impetus that had been expended in dilating them. By the contraction, therefore, of that part of the arterial system, which had been dilated by the immediate action of the heart, a quantity of blood equal to that which has been projected by the ventricle, is, as valves oppose its return into the heart, trans-

mitted with an equal force into another more advanced portion; and, in the same manner as had been done in the first, dilates and stimulates the sides of this portion. The same series of actions, with corresponding effects, is repeated to the end of the system.

Thus, after a succession of dilatations and contractions, a quantity of blood, equal to that discharged at a single impulse from the heart, is poured, at the same time, out of the innumerable terminations of the capillary arteries; and a power equal to that of the heart, is thus transmitted to that ultimate portion of the arterial system, the extent of which will be determined by the equality between the difference of its diastole and systole and the bulk of the blood projected by a single effort from the heart.

Other circumstances necessarily concur to give a direction to the motion of the blood.

The excess of capacity of the branches united, over that of the trunk from which they divide, and the continuance of that ratio to the ends of the system, must have a considerable influence in directing the course of the blood from the roots to the branches, upon the supposition of its

being effected by the vibrations of these vessels. For if a fluid be compressed strongly by the diminishing capacity of the vessel containing it, and if orifices of different size exist in the sides of this vessel, the quantity of fluid discharged from these orifices, in the same time, will, *cæteris paribus,* be as the squares of the diameters of these orifices. Upon the supposition of a synchronous contraction of all the parts of of the arterial system, the quantity of blood discharged in consequence of that contraction, from the terminations of the arteries, must greatly exceed that which would be returned to the ventricles, even if there were no valves to oppose it; the quantity which would be discharged from the former would be to that returned to the latter, as the square of the diameter of all the capillary arteries of the aortic system, to that of the diameter of the aorta alone. But it has been clearly ascertained, that the arteries constitute a cone, the apex of which is at the heart, and its broad base formed by the ends of the arteries.

The motion given to a body impelled by any point in the contracting sides of a cone, will not be in the direction of a perpendicular to the axis, but will observe an obliquity to it pro-

portionate to the extent of the angle formed by the bisection of the apex. The direction of motion will form an obtuse angle with the axis on the side facing the base. Therefore the blood will not be impelled towards the axis directly, but obliquely towards the extremities, by the contraction of the arteries.

The proportions of the cone will necessarily vary in the different states. During the systole, any section of the cone near the apex will be a less circle compared with that, formed during the same state, by a section near the base, than the former with the latter during the diastole. For a greater diminution of cavity in proportion will be required to transmit a quantity of blood through a section near the apex of the cone than would be required to transmit that quantity, in the same time, through a section near the base. Therefore, the arteries, while recovering their systole, assume a condition still more favourable to the transmission of the blood from the roots to the extremities, in consequence of this action.

If however the contractions of the arteries, as would certainly appear to be true, are not simultaneous but rapidly successive, these vessels

would, in that case be put in a condition still more adapted for transmitting a fluid from the heart to the extremities, in consequence of those contractions. For, at the instant at which any portion of the arterial system begins to recoil upon the blood, the portion next to it, on the side of the heart, will be approaching the state of greatest contraction, with its contracting *nisus* unfinished; while the vessels, on the other side of the contracting portion, will be in a state of relaxation and easily dilatable. The undulating successive manner of the arterial action, commencing at the heart, and proceeding with amazing rapidity through the arterial system, must evidently promote the course of the blood from the trunks to the branches.

From the circular and spiral direction of the fibres, composing the muscular and ligamentous coats, it is probable that, in particular circumstances, the arteries possess the power of dilating themselves to a certain degree. For these fibres by relaxation must enlarge their circle and, in consequence, augment the cavity formed by them. So far, therefore, the arteries, while they are relaxing, may possess an active influence upon the motion of the fluids they contain.

The length of the part of the arterial system, sustaining the direct impulse of the heart, might be calculated, were we acquainted with the quantity of blood projected at one impulse from the ventricles, and the difference between the capacity of the arteries in their contracted and their dilated state ; for it must extend precisely to that point between which and the heart, the difference of the arteries in the diastole and systole is equal to the mass of fluid discharged at one impulse from the heart. Every portion, then, of the vibratory system, which receives into its cavity a quantity of blood, in addition to what is already contained, equal to that discharged at a single contraction of the heart, sustains and communicates a force equal to that of the heart.

It is not to be inferred, however, that the portions of the arterial system, though their actions are not admitted to be simultaneous, are supposed to be definable by a particular point. The movements of the different portions, though not synchronous, succeed with such rapidity, that the succession cannot be discerned by the senses. The contractions of the successive portions of the arterial system, will be so blended together, that

the whole will assume the character of an undulation, commencing at the heart and advancing with the greatest rapidity to the ends of the arteries.

It has been stated, that two powers, of a different nature, the muscular and elastic, combine their efforts to produce the systole of the arteries. The arteries, perhaps, could not have been constructed with a muscular coat of itself sufficiently strong, without becoming of an inconvenient and cumbersome thickness and without being in a great measure deprived of their flexibility. Nor could the elastic power of these vessels singly have been sufficient; for nature does not supply a substance that is perfectly elastic. Had the coats of the arteries been perfectly elastic, that is, had they resiliated, from their elasticity alone, with a force equal to that by which they were distended, there might, perhaps, have been no occasion for their partaking of the muscular structure. But this being necesssarily not the case, the irritability is, with wonderful design, formed to supply the want of perfect elasticity.

Unless the actions of the different portions of the arterial system had been thus equally balanced, the circulation of the blood through them could not have been regularly performed. For, if any portion of this system had acted either more forcibly or more feebly than another portion, the blood would either have been discharged from that portion or accumulated in it; and, in either case, the circulation would have been interrupted. To prevent however the recurrence of such a condition, the constitution is possessed of a corrector in itself. For suppose, that any portion of the artery should not recoil upon the blood with the same force with which it had been distended, and that its diameter in the systole should be greater than it usually is in that state; in the next impulse that it was to have sustained from behind, the usual quantity of blood would have been thrown into this portion, the cavity of which would by that means have been distended to an unusual degree. The consequence of this unusual degree of distension would be a greater excitement in the muscular fibres, and a more vigorous resilient effort in the elastic; and, of consequence, a more vehement contraction; and in this manner would the unhealthy accumulation be removed. A similar corrective process would follow a too energetic action in any portion.

Whatever may be the objections which may be opposed to particular parts of this explanation, or whatever may be the shades of difference which may still exist among physiologists, respecting the powers by which the blood is circulated through the arteries and their manner of acting; it is certain, that as much blood is discharged from all the capillary arteries as is propelled from the ventricles of the heart in the same time; and that, therefore, the momentum of the blood issuing out of the capillaries of the larger circulation, is equal to that of this fluid at any section of the aorta itself. For, supposing the resistances in each case the same, the same force would be required to move the larger stream, with the velocity it possesses in the capillaries, as would be required to move the same extent of the less in the aorta, with the velocity which it there possesses.

Let it now be supposed, that the blood has reached the ends of the arteries, and is ready to be discharged into the concomitant veins. It is now to be circulated in a way in many respects different from that in which it had been in the arteries. The machinery and mechanism are here changed. The vessels are not of the same structure, nor

have they the same properties ; and their position, with respect to the motion of the fluid they contain, is reversed. The veins are neither irritable nor elastic ; they are very dilatable but have no re-action. They, therefore, can offer no resistance, except what may be derived from friction and position, to the blood that is thrown into them, till dilated to a great extent. What would be the effects of the action of the heart and arteries, as just described, upon the blood in the vessels in which it is now about to flow ? By repeated contractions, the arteries might continue to throw blood into the veins until the column of fluid in these vessels should balance the force of the last portion of the arteries. There are no data from which this force can be calculated. It would appear to be equal to that of the heart. To what extent, a force equal to that of the heart, distributed in due proportion, and acting upon the blood in the innumerable venous capillaries, would promote the motion of the blood in the veins, without the co-operation of any other agent, is a question we cannot certainly determine.

There are however certain reasons from which it may be concluded,—1. That the *vis a tergo*, or force derived from the heart and arteries, is

insufficient to balance and keep in motion, the whole blood in the venous system.—2. That from various phenomena accompanying this part of the circulation, the motion of the blood, as it exists in the veins, could not be produced by any power alone, however strong, that was so directed.

1. If we take into consideration the quantity of blood in the veins, it will appear too great to be sustained and kept in motion by the contractions of the last part of the arteries alone. The veins are supposed to contain at least twice as much blood as flows in the arteries. But the weight of this mass, upon the supposition of its being balanced and advanced by an impulse from the ends of the arteries, is enormously increased by the form of the vessels in which the fluid to be moved is contained, and by the position of these vessels in relation to the moving power.—The veins, as has been mentioned, ramify from trunks which arise from the heart, after the manner of the branching of a tree. The area of the transverse section of all the branches united continually increases the further this section is taken from the heart; and, at the ends of the veins, at which the impulse must be made upon the blood, the area of the transverse section is

the largest of all. The blood, therefore, in approaching the heart is constantly passing into a narrower channel. Its motion is expended not only on the column of blood before it, but upon the contracting sides of the channel along which it moves. So that, in fact, the impulse necessary for advancing the blood in the veins, would not have the weight of the blood before it, alone, to support, but the weight of a cylinder of blood, the base of which is equal in area to the transverse section of the veins at their ends, and the height is the distance between this section and the heart. The form and position of the veins are, therefore, the most unfavourable that can be conceived to the motion of the blood, upon the supposition of its being totally advanced by a *vis a tergo*.

Formerly the blood vessels were supposed to constitute a cone with the base at the heart, and the apex formed by the extreme vessels. From this structure, Muschenbroeck attempted to account for the apparent easy motion of the blood in the veins. His argument was, that, as the blood flowed from a narrower into a more ample channel, a weaker power from behind was required to advance it. The force transmitted from

the heart and arteries, would be in proportion to the square of the diameter of the small vessels, which were supposed to constitute the apex of the cone (and therefore in them smallest) multiplied into the distance of the section from the heart. But subsequent discoveries in anatomy have proved that this reasoning, though just, is founded upon erroneous principles. For it has been found that the tranverse section of all the branches united exceeds greatly the transverse section of the trunk; that in reality the heart is the apex of the cone; and that the terminations of the extreme vessels form its extended base.

The veins form frequent anastomoses. By the collision of currents meeting at considerable angles, the momemtum of each is to a certain degree expended.

2. But, allowing that the heart and arteries were sufficient to advance such a quantity of blood under all the impediments to which it is subjected, the veins are evidently not fitted for their supposed office. They must be allowed to be always in an extreme degree of distension. In the lower extremeties, particularly in the erect posture, they would necessarily have to sustain

such a degree of lateral distension as their coats could scarcely be supposed to resist. The valves, with which these vessels are furnished, admitting them to be as perfect as possible, could not remove this pressure at all times; for they must be opened by the force of the blood advancing to the heart, when the vein would have to sustain at any part the weight of a cylinder of blood, of which the base was equal to the square of the diameter of the vein at that part, and the height to its distance from the heart. But the veins, even in the lower extremities, do not appear to sustain any considerable degree of lateral pressure. When slightly pressed, they swell on the side of the point of pressure farthest from the heart, to a considerably greater size. They certainly therefore on their ordinary state are not in the situation of rigid tubes; which they must be admitted to be, upon the supposition of the blood being advanced through them by a force impressed upon this fluid at their distant terminations. Besides a vein wounded in these circumstances, would never cease to flow while there was any blood in the part of it between the orifice and the heart, which would be nearly as long as there was blood in the system.

Many other phenomena might be stated, perfectly incompatible with the supposition of the venous blood being circulated by a force acting from behind, but these we shall omit to notice at present, both because sufficient proof has been already adduced that the projectile force of the heart and vibrations of the arteries are not the only agents employed in returning the blood from the extreme vessels to the heart; and because a better opportunity will occur of considering those phenomena, after the other powers, supposed to be engaged in the circulation, have been explained.

It may be concluded, therefore, from what has been said, that a certain degree of the motion of the blood in the veins is produced by the force of the arteries; or, in other words, by a *vis a tergo;* but how great we cannot certainly estimate;—that, in consideration of the mass of fluid to be advanced, and of the obstacles of various kinds and of powerful efficacy which are to be overcome, the supposition of the whole motion of the blood in the veins being the effect of an impulse received from the terminations of the arteries appears most extravagant and altogether untenable; that the structure and position of the veins are not fitted for circulating a fluid, in consequence of a

cause acting at the commencement of its motion alone; and that we are convinced that the venous blood is far from being wholly circulated by such a cause from various phenomena attending that circulation.

Some physiologists still contend, that the coats of the veins are contracted and dilated alternately after the manner of the arteries; but that, as the blood flows with much less impetuosity in the veins than in the arteries, the small imperceptible vibrations of the former are sufficient for the accomplishment of an effect proportionably less.

In the first place, what is assumed in this argument is erroneous, that the quantity of motion belonging to the blood in the veins is less than that belonging to the blood in the arteries. For, it has been already demonstrated, that the quantity of motion in equal portions or lengths of the circulating system, supposing all the branches included, is equal in equal times, from whatever part of the circulation those portions may be taken. Now, as the distance from the ends of the arteries to the heart, may be presumed to be equal to the distance of the ends of the veins to the heart, it must follow, that the quantities of

motion, in the two systems embraced between these equidistant points and the heart, are universally equal in equal times.

Supposing the motions in the two systems independent on each other and the resistances the same in both, the powers required to produce those motions would be equal. The venous circulation receives indeed an impulse from the arterial, the extent of which impulse we cannot ascertain. But on the other hand, it must be admitted, that the formation of the veins, and their position with respect to the moving powers are less favourable to the motion of the venous blood than those of the arteries to the arterial, upon the supposition of each being produced by an impulse impressed upon it from behind, or by the contraction of the coats of the containing vessels. We cannot compare the impetus received by the venous circulation from the arterial with the additional resistances to be overcome in the veins, but, I presume, it will be found to be erring in favour of the arterial impetus, if we suppose them equal; and that there still would remain a motion in the veins requiring powers equal to those by which the whole arterial blood is moved. If such vigorous and perspicuous causes are

required to produce the arterial circulation, can we suppose an equal effect, the venous circulation, to be produced by causes of the same kind, and operating in the same manner, but so much less in degree as to be perfectly imperceptible?

It is admitted that vibrations are perceptible in the larger arteries only, but it is assumed that the smaller or capillary arteries also vibrate; and that, in consequence, they contribute as well as the larger arteries to promote the passage of the blood through their cavities. May not, it has been asked, the capillary veins also vibrate, though imperceptibly, to the same extent with the capillary arteries; and equally promote the current of the blood while it flows in them?

As the capillary veins are the continuation of vessels which certainly do not vibrate, it is presumable that they are possessed of the same structure and properties. Inflammation, while it affords the strongest proof of the vibration of the capillary arteries, seems to dispossess the veins of that property. For if the capillary veins were excited by stimuli in the same manner with the capillary arteries; and if, when excited,

they affected the blood in the same manner that the capillary arteries in that condition are allowed to do, the veins of any part strongly stimulated, would have transmitted the blood as rapidly through them as it had been received from the excited arteries, and there would be no accumulation, and of consequence no inflammation in that part.

But admitting that not only the capillary veins but also that the whole venous system vibrated, what would be the effect of these vibrations?—Undoubtedly to send the blood from the heart to the extremities; from the apex to the base of the cone; to reverse the course of the blood, not to promote it.

The quickened circulation and strong beatings of the heart and arteries which attend vehement bodily exertion, readily indicated muscular action to be connected with the motion of the blood; and have caused it, in the general opinion, to be ranked among the powers provided by nature for the circulation of this fluid.. The following is the manner in which this power is supposed to contribute its aid towards the accomplishment of this important end. A muscle, in its contracted

condition pressing upon the full veins that run beneath it, drives the blood from the parts of the veins that are under the influence of its pressure. The blood will tend to flow in all directions from the centre of pressure, but is prevented from returning towards the extremities of the veins, by the valves with which these vessels are furnished; and is necessarily directed towards the heart, in which course it meets with no obstruction. As the muscle soon relaxes, the vein is again relieved from pressure; becomes an easy receptacle for a new supply of fluid, which by the mechanism of valves can only be drawn from the extremities and which the succeeding contraction of the muscle directs in the same forcible manner towards the heart.

Let us consider for a moment more closely the effects of muscular action upon the motion of the blood. Admitting that the valves with which the veins are furnished are sufficiently perfect, which they certainly are not always, to prevent the retrograde motion of fluids, it appears doubtful whether the circulation of the blood be directly promoted by msucular action. The same agent, which drove the blood, flowing between the center of pressure and the heart, with aug-

mented impetuosity towards that organ, would close the valves against the blood that was coming from the extremities, and bring for a time to rest the whole current that was in motion between the shut valves and the ends of the veins.—Reasoning therefore *a priori*, we should be led to expect that the action of muscles would impede the circulation in one part while it increases it in another, and that the immediate influence of their action would be to render the circulation unequal in force and interrupted, not uniformly stronger and quicker.

This argument receives considerable support from the frequently observed effects of violent muscular action. In those spasmodic affections unaccompanied with any increase of the natural heat, as epilepsy, hysteria, and the like, where the muscles are contracted and relaxed with great rapidity and in a violent manner, the motion of the blood does not appear to be increased; the pulsations of the heart and arteries are found to be unequal both with respect to strength and quickness, not generally stronger or more frequent.

After long voluntary exertion, and when they have been so frequently repeated as to produce

fatigue, muscular contractions cease to have any influence in augmenting the motion of the blood.

During voluntary bodily exertion, two causes seem to aid at least the direct influence of muscular action in increasing the motion of the blood. By the stronger, though interrupted, impulses of the returning blood, the heart sustains a degree of violence, the auricles are perhaps more than usually distended and stimulated, and the whole organ roused into a more vigorous action. The increase in the energy of the heart, which will speedily be communicated to the whole circulating organs, continues, though the cause that produced it may be withdrawn. Hence we find that a strong momentary effort is often succeeded by violent pulsations, which frequently continue for a considerable time, though the cause that produced them be not repeated, and though bodily exertion of every kind, be entirely suspended. Chemistry has lately supplied another cause, why the circulation should be increased by violent muscular action. In making any great bodily effort, the chest is naturally enlarged to an unusual degree, the inspirations are fuller and often more frequently repeated. Hence a greater quantity of atmospheric air will be received into,

and transmitted from the lungs in the same time, than when the body is at rest. The heat, therefore, of the system will be increased, the blood rendered more arterial and stimulant, and the general irritability augmented. A stronger and more rapid circulation will necessarily be produced by a combination of causes increasing at the same time the irritability of the circulating organs, and the energy of their appropriate stimulus.

In fevers and inflammatory diseases, while all the muscles, excepting those employed in respiration, are at rest, the heart and arteries often beat as powerfully, and as rapidly, as they are ever observed to do under the most violent exercise.

Since therefore, the blood often circulates with its greatest force and regularity, for a considerable period, during which nearly all the muscles of the body, but especially those which by their action could affect the vessels, are at perfect rest; since a quickened circulation is not always the consequence of a violent and very general action of the muscles; and since, even in those cases in which muscular action is attended with a more vehement circulation, this effect is chiefly to be

ascribed to causes different from the immediate action of the muscles upon the subjacent vessels; it may be concluded with the most perfect certainty, that the action of the muscles super-incumbent upon the veins is not necessary to the circulation of the blood. Nor was it indeed to have been expected that nature would have left any share of an operation, that cannot be interrupted scarcely for a moment without the suspension or loss of life, to a cause so accidental and precarious.

The important uses of muscular action in removing impediments, in restoring the balance and in correcting errors in the circulation, might easily be explained; but as these are only occasionally required, and presuppose the existence of powers adequate in ordinary circumstances to the motion of the blood, it is foreign from the purpose of this inquiry to dwell upon them.

A cause of the motion of the venous blood, more constant in its operation than muscular action, but similar in its manner of application, is supposed to be derived from the vibrations of the arteries. It will be remembered from the account that was given of the structure and arrangement of the

blood vessels, that the arteries are generally accompanied by their corresponding veins. The arteries by dilating are supposed to make a depression into the sides of the accompanying veins, to diminish their cavity, and to promote the course of the blood contained in them towards the heart, in the same manner that it has been believed to be promoted by the pressure of a contracting muscle.

Nothing could more strikingly demonstrate the difficulties to which physiologists have been reduced in attempting to account for the motion of the blood in the veins, than the efficacy which has generally been acknowledged to belong to this agent. As we may fairly suppose that an artery in its diastole becomes distended equally on all sides, the difference of the length of its semidiameter in its diastole and of that in its systole will measure the whole depth of the depression which by its dilatation it can make in the vein.— As both the artery and the vein are cylinders, and mutually recede on each side of the line of contact, the diminution produced in the general cavity of the vein by so imperceptibly shallow and narrow a depression must be extremely small. But to allow any effect, in the way con

tended for, to this impulse, it is necessary to suppose the veins to be in a state of rigid distention, which they certainly, for the most part, are not; and if they were in such a state of distension, would not the dilating artery be more likely to displace the vein slightly, which in general is not difficult, than to make any impression upon its sides? But we have hitherto been supposing the artery to be throughout in close contact with the vein. This is not generally the case; and if they were in contact, would it not be as reasonable to suppose that the artery receded from the line of contact in its systole, as that it advanced beyond it in the diastole? Granting that the artery when dilated made an impression upon the vein, throughout its whole length, the effect of this impression would be as much against as in favour of the motion of the blood in the vein.—For in fluids the momentum is equal in all directions, from the centre of pressure. It will scarcely be contended, that the valves could have any influence in this case. Before any alteration could be produced by them in the direction of the impelled fluid, it is necessary to suppose them to have been closed; and to close them the blood must have been driven on one side in a retrograde course. But these are effects to which

the slight impulses of an artery are, in any supposeable circumstance, altogether inadequate.— Besides, neither in the quiescent vein exposed by the dissecting knife, nor in the uniform stream flowing from an orifice made into its cavity, can the senses recognize any marks of the pulsations of the arteries.

But it may be urged that, though the reasoning here pursued is confirmed by observation, and conclusive so far as the larger vessels are concerned, the extreme vessels are so minute, so multiplied, and so accumulated, that a capillary vein may be acted against by several capillary arteries at the same time. In reply to this observation it may be stated, that between the capillary ramifications, if we judge from analogy of structure, some muscular, cellular, or nervous substance in all probability always intervenes, by which the vessels are protected against the actions of each other. But in a case in which observation cannot be appealed to, and where argument may be opposed to argument without producing conviction, let it be admitted that the venous capillaries are acted upon by the arterial to the extent and in the manner here supposed. What would be the result of these pulsations? The general cavity of the venous capillaries is sup-

posed to be diminished by the pressure of the dilated arteries. In what direction would the greater part of the blood expelled from the venous capillaries flow? The discharge, supposing no other power concerned, would be in proportion to the dimensions of the outlets afforded by the vessels under pressure. But, as the veins form a cone, the apex of which is at the heart, the outlets would be greater beyond the pressure, or at the ends of the veins, than on this side of it. Therefore, by the supposed pressure acting singly, a greater quantity of blood would be driven in a retrograde direction than towards the heart. The momentum, consequently, communicated to the venous blood *a tergo* by the impulses of the heart and arteries, would be diminished by the supposed lateral pressure of the arteries. This argument opposes with equal force the supposition of the venous blood being aided by the proper vibrations of the veins.—Admitting still that the venous capillaries, either by their own action or by the borrowed impulses of the accompanying arteries, contributed in some unknown way to the passage of the blood through them; all those arguments which have been advanced in refutation of the opinion of the blood in the larger veins being circulated solely by the

force of the heart and arteries, prevail equally against the supposition of its motion being all effected by these organs, aided by the proper or borrowed action of the capillary veins.

Capillary attraction is another power to which no small influence in advancing the venous blood has been attributed. It is certain that fluids will rise to a considerable height in tubes by what is termed capillary attraction; and, as the small extreme veins are in capacity similar to capillary tubes, it is supposed by some that the blood is circulated through the veins partly by this cause. Various hypotheses have been proposed respecting the cause of the ascent of fluids above their level in capillary tubes; but, without waiting to examine these hypotheses, there is no doubt that the effect is produced by an attraction in some way exerted between the sides of the tube and the fluid. It is therefore plain that any force which tends to transmit the blood through these small vessels must act in opposition to any attraction between the sides of the vessels and the fluid that is propelled through them, and that, of consequence, that force, which is admitted to proceed from the arteries and affect the blood in the small veins especially, must be resisted by capillary attrac-

tion. It is evident, therefore, that capillary attraction, if it contribute any assistance to the passage of the blood through the small veins, is the only power concerned, and that it cannot co-operate with any other. But further, as both ends of these capillary tubes are inserted into, or arise from, vessels filled with blood, what reason is there to suppose that the blood should always be moved in one direction by this power? Might it not be apprehended that its motion would be occasionally inverted by it? After these vessels have been all filled with blood, by what power are they to be again emptied and fitted for the reception of a fresh supply? Besides, in the circumstances most favourable for the operation of capillary attraction, fluids can be raised by it only to a limited height above their level.—Capillary attraction, therefore, instead of promoting the motion of the blood through the small veins, appears to be a principal cause of its retardation.

The motion of the blood has been supposed to be influenced by gravitation. The argument usually urged upon this subject is, that this power operates upon the blood as it does upon all the substances with which we are acquainted,

that gravitation will sometimes favour, and sometimes retard, the motion of the blood according to the position of the vessels in which it moves, and that if this power should favour it in one part of the course it is likely to retard it equally in another, or in vessels in which its current is reversed.

Dr. Darwin, after acknowledging that the projectile influence of the heart and arteries does not extend beyond the ends of the arterial system, maintains that the veins take up the blood from the arteries by absorption, in the manner that a sponge imbibes water. But this celebrated author has not waited to reply to a question that seems to follow of necessity—What is the cause of this absorption?

Dr. Wilson, formerly a physician of Newcastle upon Tyne, published a very curious and ingenious pamphlet on the causes of the motion of the blood, especially in the veins. This physiologist conceives that the heart is dilated by a property inherent in itself, and not by the impetus of the blood either issuing from the roots of the veins into the heart, or from one chamber of this organ into another, and that, in consequence

of this dilatation, a cavity or vacuum is produced, by which the blood is pumped from the veins into the heart. The question respecting the dilatation of the heart had been a subject of controversy for a considerable period after the discovery of the circulation of the blood. It had been urged against the opinion now renewed by Dr. Wilson, that the natural state of the muscles was that of contraction; that the heart being a hollow muscle and having no antagonist, the constant tendency of its own action must be to diminish its cavity; and that, even admitting the diastole to be the natural condition of the organ, no cavity or vacuum could be formed but simply a relaxed extension of the sides, placing them, at most, in a situation more favourable to be influenced by the impetus of the returning blood. Dr. Wilson has ascribed to the heart the property of self-dilatation without supporting his assertion by adequate proof, or without refuting the arguments which had been urged against it, and which had long been admitted in the schools of medicine as irrefragable and conclusive. In consequence, the hypothesis of Dr. Wilson does not seem to have attracted much attention; and the doctrine that the cavities of the heart are dilated by the force of the blood rushing into them con-

tinues to be received without hesitation, and almost without inquiry.

The vitality of the blood and of the vessels in which it flows, independent of the action of those vessels, has been supposed by some writers, not only to generate an increase in the motion of this fluid, but so to modify its character, as to emancipate it, in a great degree, from those laws to which the motion of dead substances is subjected. "It is in vain," says a recent author, "to attempt to apply the laws of hydrostatics to the blood, a living fluid moving in living tubes." The late Mr. John Hunter supposed that the blood was possessed of life in a more extended sense than had been done by former writers; and has attributed to it faculties, which necessarily imply the existence of a certain degree of consciousness, judgment, and even locomotion. But if the doctrines of this celebrated author respecting the life of the blood be divested of all that is purely hypothetical, they will be deprived of the greater part of their originality. An opportunity may occur in the course of this inquiry, of considering some of these doctrines at greater length.—No physiologist, nevertheless, so far as I know, has ventured to describe how the passage of the

blood could be immediately promoted, or its motion modeled by its vitality. The assertion of a late writer, indeed implies the belief of a voluntary effort on the part of the blood itself, since he enumerates among the causes of the circulation of this fluid, the frequency with which it had travelled the same course before. But admitting the truth of all that has been said, or fancied, respecting the vitality of the blood, there would still remain insuperable objections to the supposition of the motion of this fluid being promoted by it in any degree, or being regulated by peculiar unknown laws. Should the property of locomotion be ascribed to single particles, or to any combination of particles, there would still be required a general mind to direct the wills of all those particles, or combinations of particles, composing the whole mass, to one common object.

With respect to vitality considered as a cause of fluidity, or of that particular combination of the constituent parts, by which the uniform consistence of the blood is preserved, and by which it is fitted for motion in vessels of minute calibre, or as assisting in the formation of those stimulant qualities, by which the heart and arteries are ex cited, and the motion of the blood indirectly pro-

moted, the particular point of the question now under discussion, is not at all concerned.

But it is contended that the organs, in which the blood is contained, and by which it is put into motion, may, in consequence of their vitality, impart to this fluid a peculiar motion, different in its properties from that of fluids propelled by inanimate agents. To such as argue in this manner it will be sufficient to reply, that the motion of bodies, unconnected with the causes by which it may have been produced, is the simple object of mechanical philosophy, and that motion in all cases observes certain fixed laws, is as soon as produced independent of the causes by which it had been generated and can be in no degree afterwards modified by them without the communication of a fresh impulse to the moving body. The hand from which the ball has just been discharged, has now no influence in quickening or retarding its progress through the air. In the same circumstances, the motion, that had been generated by any living powers, will not be expended more or less rapidly than the same motion generated by inanimate agents. Nor can the qualities of motion be separated. It is not possible for any power to communicate to any body

one quality of motion and withhold from it another. It cannot communicate to a given quantity of matter a certain velocity, and withhold from it a due proportion of momentum, nor momentum without a requisite velocity.

Opinions, nevertheless, contradicting these plain and well-established truths, have been maintained by men of eminence. The ingenious and accurate Dr. Hales even, having in the course of his valuable experiments to ascertain the force of the blood, found, that the momentum of this fluid issuing out of a divided vein bore no proportion to that of the stream from its corresponding artery, (while considering the quantity of fluid in motion, and the velocity with which, in the living body, it must flow, in order to return a sufficient supply to the heart in the time required, the momentum of the stream from the vein ought to have been nearly equal to that of the artery,) concluded hastily, that the motion of the blood was not governed by those laws to which the motion of other fluids was subjected. But would it not have been more becoming and judicious in the philosopher to suspect that his experiments did not fairly indicate the relative forces of the venous and arterial currents, than

that the laws of nature, so uniform and constant, should, in this instance, be transgressed? Before a doctrine so contrary to experience in every other subject; so contradictory to reason; so discouraging and inauspicious to improvement, be received, it will be necessary for those who have advanced it, to shew that they are acquainted with all the capacities of the system, and that nothing can have escaped their penetration. For their argument is not positive but negative. It is founded upon the supposition that every thing that is possible in the case is known, a species of reasoning which, particularly, in physics, has ever been found to be fruitful in error, and which ought never to be relied upon except, perhaps, in the science of pure mathematics. On the contrary, it is hoped, that in the course of this inquiry it will be found, that the experiments of Dr. Hales and other eminent inquirers, do not sanction the opinion that the motion of the blood is at variance with the received laws of hydraulics, but that, like that of every fluid it is subjected to their government.

With the cause of muscular action we pretend not to be acquainted, but the effects of that action, as exhibited in the motion of material sub-

stances are the objects of as certain calculation as the apple that drops from the tree, the ball that is projected from the mouth of a cannon, or the moon that revolves in its orbit, and we are as little acquainted with the causes of gravitation, attraction and projection, as with those of muscular action.

PART II.

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It has, I trust been clearly shewn, that the causes enumerated in the preceding pages are inadequate to the whole motion of the blood and that powers to which an important share of this effect is to be attributed, remain to be explored. We proceed then, in the second part of this inquiry, to the developement of those other causes which appear to contribute to the motion of the blood, and which co-operating with the former seem adequate to that effect.

As the heart is unquestionably the most important organ engaged in the circulation of the blood, it will be necessary to resume the consideration of its movements.

It will be found, on reference to the preceding part of this inquiry, that the heart is divided into what is properly termed the body of the heart and the appendages; that each of those divisions, consisting of two chambers, assumes successively

opposite conditions; and that the diastole of the ventricles is synchronous with the systole of the auricles.

It is necessarily inferred from the conditions of the circulation, that each of these chambers, in assuming the systole, discharges a certain quantity of blood, and in recovering the diastole, it receives into itself again a quantity, in ordinary circumstances, equal to that which it had discharged.

It was also stated, that physiologists are agreed respecting the causes of the contraction of the heart, and are unanimous in ascribing this movement to that property, which characterizes muscles (the class of substances to which the heart belongs,) of being contracted upon the application of appropriate stimuli.

The peculiar stimuli by which the different parts of the heart are excited to contract themselves, are without doubt, derived from the fresh blood which they receive at every diastole, and, perhaps in the most extensive degree from the uneasy distension sustained by the muscular fibres at the moment of full dilatation.

By what causes are the chambers of the heart dilated after contraction? This question has particularly engaged the attention of physiologists, from the period at which the circulation of the blood was discovered; has given origin, as we have seen, to various hypotheses; and, in the opinion of many, is still unresolved.

As the determination of this question is of the utmost importance in the present investigation, it seems proper, in this place, although some repetition may be involved in the statement, to consider briefly the causes to which the expansion of the chambers of the heart have been imputed.

The doctrine maintained on this subject by the celebrated Harvey, beyond whose age it is unnecessary to refer, was, that the auricles were dilated by the force of the blood returning to them through the veins, and the ventricles by the momentum of the blood projected through the auricular passages, during the contraction of the auricles. But the opinions of Dr. Harvey, on this subject, have not been universally adopted. It has appeared to many an extravagant supposition that there should still belong to the blood

in the venous trunks such a force, especially considering the supposed origin of it, as should be sufficient to expand any of the chambers of the heart; and, still more so, that the left ventricle should have for its antagonist so thin and slender a muscle as the left auricle. It has been contended by many that the property of dilatation, as well as that of contraction, is inherent in the heart itself, and that the blood does not force a passage into the auricles or ventricles, but enters them without opposition, or is solicited into them by their previous dilatation. Dr. Browne Langrish, is one of the most able supporters of this doctrine. This philosopher contended, that two kinds of muscular fibres are interwoven in the fabric of the heart; that by the contraction of one set of fibres the cavities are diminished; that, as soon as these have ceased, the other set begin to act, and by their contraction restore the former fibres to the distended state; and that thus the chambers are dilated.

It was urged in opposition to this doctrine that the assumption of two kinds of fibres existing in the heart and acting at different instances was purely hypothetical; that the supposition of muscular fibres apparently of the same structure and

properties, forming the same muscle, and subjected to the same exciting causes, acting at different times, was unreasonable; and that the contraction of any of the fibres of a hollow muscle, must necessarily diminish the cavity still farther, instead of expanding it. These objections appeared to afford a satisfactory refutation of the opinions of Dr. B. Langrish; and as has too frequently been the case, the overthrow of this doctrine was considered as an irrefragable proof of the truth of the other.

M. Dumas, a French physiologist of great celebrity, clearly discerned the insuperable nature of the objections which may be opposed against every hypothesis hitherto advanced on this subject, and has lately presented a new view of the causes of the expansion of the heart. He maintains that there constantly surrounds the blood in the living system, an expansive elastic emanation or atmosphere *(une expansion active)* so powerful as to expand the cavities of the heart when they are in the state of relaxation which follows contraction; and that this expansive faculty is not only the true antagonist of the muscles of the heart, but the cause of the pulsations of the arteries.

But this elastic emanation, to which the philosopher has attributed such wonderful powers, is purely hypothetical; its existence, indeed, has been attempted to be proved, but by the most vague and remote analogies, and inapplicable and deceptive experiments. But admitting that it did exist, it must necessarily surround the venous as well as arterial blood, and therefore, the veins must be allowed to be constantly in such a state of distension as they would be placed in by a power capable of dilating the left ventricle of the heart; a supposition contradicted by the slightest observation.

The doctrine generally delivered in the schools of medicine, at present, on this question, is, that the auricles are dilated by the force of the returning blood, and the ventricles by the impulse transmitted to them through the medium of the blood by the auricles; both ventricles and auricles being placed by the relaxation following contraction, in the situation most favourable to be dilated.

For the purpose of obtaining a satisfactory solution of the question—"What are the causes

of the dilatation of the heart?"—a solution of the first importance in the present inquiry, it will be necessary to examine, with considerable minuteness, the structure and mechanism of the heart itself; its situation in the chest, and the relation it may bear to the neighbouring parts, which will involve in some degree the consideration of the structure and mechanism of those parts; the momentum of the blood in the venous trunks, and the sources from which that momentum is derived.

The heart, as has been stated, consists of two parts, the body of the heart or that part containing the ventricles, and the appendages or auricles. The body of the heart is a strong hollow muscle. Many attempts have been made to explain the origin, course, and insertion of the fibres of this muscle. Dr. Alexander Stewart, a physician who flourished about the beginning of the last century, used a method well fitted for ascertaining the muscular structure of the heart,—that of preparing hearts by boiling. The hearts of bullocks, sheep, and other animals, he treated in this manner. According to Dr. Stewart, the body of the heart consists of a single muscle, which when expanded or stretched out takes the

form of a semicircle, and of which the fibres are all parallel to each other and to the base of the semicircle. The hearts, which had been previously prepared for his purpose by boiling, he unfolded in the presence of the Royal Society; which, as is well known, was originally instituted for the purpose of making experiments, as well as for that in which it is now usually employed in reading and hearing communications. He commenced at the raphe, and separating carefully the outer layer, which was easily distinguished from that beneath by the different direction of the fibres; and pursuing a course in which he was conducted, as it were by a thread, he easily unravelled the whole; and found that it resolved itself into the shape and structure just mentioned. He attempted to illustrate the mechanism more clearly by shewing how this muscle might be refolded into its original form. The manner of refolding the muscle may be represented by a diagram. Suppose (fig. 2.) A B C to represent an ox's heart that had been prepared and unfolded after the manner described. Let A B be the diameter and D the centre of the circle of which A B C is the semicircle, and let f f f f represent the fibres of which it is composed running parallel to each other and to the diameter A B. Let the

radius D B be supposed to be folded over in such a manner that D B shall cross the fibres f f f f nearly at right angles and the point B fall upon the circumferences at E; the part E D B of the semicircle forming a hollow cone, of which the apex is at D, and of which the base is inclosed by E D bent into a circle. This may fitly be compared to a funnel formed by the folding of a piece of paper. Then by continuing to roll until the whole muscle is folded, two rotations are supposed to be completed. The radius A D, falling upon the external surface of the cone, will, in consequence of the conical shape of the figure, and of the swelling and convexity of the sides, be found to run very obliquely between the apex and base; and the point A will fall considerably within the circumference. The line A D will represent the direction of the raphe on the external surface of the heart. The left ventricle is inclosed by the first rotation of the muscle. After the second rotation has commenced, the muscle continues for a space separated from the first; but at length becomes attached to it; leaving a space between the outer surface of the first rotation and the inner surface of the second for the formation of the right ventricle. The walls of the left ventricle at every

place, except at the septum or portion forming the middle partition between it and the right, are strengthened by two convolutions of the muscle, while those of the right have every where only the support of one. Lower, an ancient anatomist of great celebrity, the same who inspired mankind with the hopes of exchanging the decripitude of age for the vigour of youth, and even of immortality itself, by his success in transfusing the blood of one animal into another; this anatomist bestowed great labour in the examination of the structure of the heart; he pursued the same mode of investigation which was afterwards adopted by Stewart; that of unravelling the fibres of the hearts of bullocks and of sheep, which had previously been prepared by boiling, and arrived at the same conclusion: "From which," says he, " it is plain that the fibres of the inner and outer walls are the same and continued, although they seem to proceed in a manner contrary to each other." Any person may soon be satisfied of the truth of this description by repeating the simple experiments of Stewart and Lower. One thing which seems to have been omitted in the account given of Stewart's experiments, and which appears in the plates of Lower, is, that some of the fibres composing the outer

layer upon reaching the base, turn inwards and pursue their course round the mouth and the internal surface of the ventricles. The fibres are not all of the same length. Those which in the extended muscle were most distant from the base being shorter than those which were nearer to it. Some of the fibres appear not to have proceeded their whole course but to have turned inwards at different distances to form the fleshy columns which characterize the cavities of the ventricles. The fundus of each ventricle is furnished with ligamentous substance, surrounding it and also running across and dividing it into two openings, the one into the corresponding auricle, the other into the trunk of one of the great arteries. The muscular fibres are firmly attached to this ligamentous substance.

Several important consequences are deducible from the form and position of the muscular fibres of the heart. In the first place, all the fibres every where, except perhaps the septum, are more or less bent into arches. The form which the fibres bear in the heart may be allowed to be that which is natural to them and that which each would preserve when separated from its fellows, without any injury having been done to its struc-

ture, and freed from the agency of foreign bodies. Let us consider then the effects of contraction and relaxation upon a fibre of this form.

But without hazarding any opinion respecting the causes of muscular action, it may, in the first place, be requisite to state certain properties which are known or may be conceived of necessity to belong to it. In the first place, the state of relaxation is the natural, and that of contraction the forced, state of the muscular fibre. This is generally admitted by physiologists and is allowed to be true, particularly with respect to the fibres of the heart, as will afterwards more clearly appear. It is certain also that when a fibre is contracted, the centres of the contiguous particles must approach nearer to each other, and that they must again recede when it is relaxed. As every very small portion of a fibre possesses, as completely as the whole, the muscular structure, it must also possess completely the muscular properties, and must act according to the laws which regulate the movements of the whole, independently of any other part of the fibre.

Let A B C (fig. 3.) then represent the portion of a muscular fibre of the heart in its natural

situation or dilated. Suppose A d, d e, e f, f g, &c. to mark very small portions of this fibre. Let the fibre be supposed to contract. All the minute portions A d, d e, e f, f g, act independently of each other; that is, though all the other portions were removed, any very minute portion, possessing the same properties and subjected to the same influences would act according to the same manner. The particles forming the two very minute portions A d, d e, would approach each other in the direction of a straight line passing through their centres, or in the direction of the line in which they are placed. The same will happen in the portions d e, e f, f g, &c. and after contraction, every particle will have the same position relatively to the particles contiguous, and to every other particle, that it had before it was contracted, and the whole A B C will become a similar portion of a less figure of the same species. A B C, if the segment of a circle, will become a b c, the similar segment of a less but concentric circle. In the relaxation of the fibre the particles or globules must recede from each other in the direction of the straight line passing through their centres. Upon the supposition that nothing opposed the natural tendency of the fibre which formed the segment of the circle, a b c

by relaxing would become A B C. Whatever then may be the curve of which this fibre forms a segment, the effect of contraction would be to form a similar segment of a less curve of the same kind, and that of dilatation a similar segment of a larger curve of the same kind. The change in the fibre produced by dilatation may be illustrated by comparing it with the effects of heat upon a metallic hoop; the segment of the hoop if heated equally will become the similar arch of a larger circle.

An elastic substance is in the heart every where mingled with the muscular, and no doubt contributes greatly to fix the form of the muscular fibres. The elasticity may be constituted in such a manner as either to co-operate with, or to resist the contractile effort of the muscle.—For reasons which will afterwards appear, it is assumed that the resilient power is opposed to the contractile. As the natural position of the elastic substance in every fibre must be the same with that of the muscular, any alteration in the elastic fibre, produced by the muscular portion acting according to its natural direction, must be to contract or expand the elastic according to its natural direction. Therefore the elasticity if it

is an antagonist power to the muscular, must tend to restore the fibre to the same position relatively that it had before it was contracted.

In consequence of the parallel direction of the fibres of the muscle by the convolution of which the heart is constructed, it necessarily happens that every fibre composing the first rotation is concentric with all those, the circumferences of which lie in the same plane with itself: that the circumferences of all the fibres are placed in planes that are parallel to each other, and that the fibres of which the circumferences are in one plane are similar, and placed similarly to the fibres the circumferences of which are in another plane. A straight line, therefore, drawn from the apex to the base of the cone, and passing through the centre of position of any one circular fibre which has performed the first rotation, will pass through the centre of position of all the fibres composing the first revolution of the muscle. The same must evidently be the case with respect to the fibres of the second rotation. As the muscle when performing the second rotation has its lower superficies adapted to the upper superficies of the first rotation, it follows, that the straight line, which passes through the centre of

position of all the fibres of the first rotation, will pass through the centre of position of the fibres of the second rotation. Hence it occurs, during the contraction of the muscle of the heart, that the change produced in the figure of any one fibre is accompanied by corresponding changes in that of the rest; that the movement of one fibre is no where counteracted by that of another, but supported by it; and that there is a general, free co-operation in all the fibres tending to the production of one effect, which is the diminution of the cavity of the heart. This is plainly maintained by all the anatomists who have investigated most carefully the structure of the heart, however far they may differ in some particulars, as Lower, Stewart, Winslow, &c. "We must see clearly," says Winslow, "that the whole structure tends to make an even, direct, and uniform contraction."

It follows also, from this arrangement of the fibres of the heart, that perfect freedom is afforded to all the fibres to assume that position and form to which they are inclined by their natural structure. The natural tendency of the fibre to enlarge uniformly its circumference is every where supported by a corresponding tendency in the other fibres.

There arises, therefore, from the circular direction of the fibres of the heart and the arched form of its walls, a very remarkable property, that of dilating or expanding its cavities by the simple relaxation of its fibres.

The conclusion, which appears so clearly deducible from the structure of the heart, is confirmed by observation. The hearts of frogs, newts, insects and other cold blooded animals, which, as is well known, are more tenacious of life than the more perfect animals, continue to beat for some time after the extraction from the body. This action may be greatly prolonged by placing the hearts which are the subjects of observation, upon a warm stone or immersing them in warm water. The dimensions of the organ will be observed alternately to be increased and diminished. I put the hearts of some frogs just extracted from the body into water blood warm. They were thrown into violent action, and, upon some occasions, projected a small stream of a bloody colour through the transparent fluid. It was supposed that a stream of the same kind continued to be projected at every succeeding contraction, but that, after the first or second, it ceased to be observable, in consequence of the liquid sup-

posed to be imbibed and projected, loosing its bloody tinge, and becoming transparent, or of the same colour with the fluid in which the heart was immersed. The organ was felt by the fingers to expand during relaxation. The ventricles are generally distended with blood after death. These observations prove not only that the heart is, to a certain degree at least, dilatable, in consequence of the relaxation of its fibres, but also that the relaxed state is that which is natural to it. If the contracted were the natural condition of the muscle, the heart, after it had been abstracted from the body, and therefore placed without the influence of those powers which might be supposed to dilate it, would for ever remain contracted; and the application of fresh stimuli would only tend to confirm it in that state. There could be no motion observable in the heart after its abstraction from the body, if the systole were its natural condition.

It may safely be inferred that the ventricles of the heart, during the act of relaxation, necessarily become dilated or expanded, but to what extent or with what force, we have no means of ascertaining. The power, required to bring a single curved fibre into a straight line, would no doubt

be inconsiderable; yet as all the fibres composing the substance of the heart, are made to act together, and to conspire during all their action to one common effect; and as, from the attachment of the fibres, no one fibre can be displaced or altered in its figure, without at the same time deranging and pressing into a form adverse to their natural structure, a number of others, it must be admitted that the heart would retain its shape and preserve the form and capacity of its ventricles during the state of relaxation in opposition to no inconsiderable powers. The convexity of its external surface and arched form of the walls inclosing the ventricles would add greatly to the capacity of preserving its shape unaltered by powers acting against its external surface, especially if those powers were directed equally against the whole of this surface. The faculty which the heart possesses of enlarging its cavities after contraction, in consequence of the causes we have been endeavouring to explain, must be increased by the spirit and energy of life, to a degree of which we can form no adequate conception by an examination of the dead fibre.

It will on all hands be admitted that the peculiar arrangement of the muscular fibres which

has been described, must place the heart after contraction in a condition of being much more easily dilated than would otherwise be the case, by powers acting in conformity with the natural tendency of those fibres.

It will be found, by varying slightly the angle made by the radius of the muscle with the parallel fibres across which it is folded, that not only will a great variety be given to the form of the heart, but a diversity to the angles which the fibres of one convolution will make in crossing those of another. Hence is explained the diversity that is found not only in the shape but also in the direction of the fibres of the hearts of different animals. This mechanism will give all that vast variety and apparent intricacy to the direction of the muscular fibres which are actually found in the structure of the heart.

The auricles, or what has been termed the appendages, are placed near the fundus of the heart. They differ greatly in appearance and structure from the ventricles. Their dimensions are small in comparison with those of the body of the heart. The auricles are flat, earlike, notched in the edges, depressed, not convex in

the dead subject like the ventricles. Their substance is thin; small bundles of muscular fibres are placed in the form of rings, or portions of rings, inclosing each other, with furrows appearing particularly in the internal surface between the rings. At the bottom of these furrows, the substance is so thin as to be almost transparent. As the membrane between the muscular bundles is very expansive and yielding, the auricles are easily dilated by any force acting against their internal surface; and, upon the withdrawing of that force, readily fall into the collapsed flat condition. The auricles are not spherical but form a cavity by the dilatation of one side principally. From the position and form of the muscular fibres, it does not appear that their relaxation would of itself dilate the cavity in any degree.

There is no communication of substance between the auricles and ventricles. The fibres composing the former do not pass into the latter, nor *vice versa*. For, if the heart be boiled for a proper time, the auricles will drop off or be easily separated from the ventricles, without any rupture of muscular fibres.

At the openings between the auricles and ventricles, valves are stationed, the offices of which have been already described.

As the heart is placed in the chest, and as its motions will be found to be materially influenced by its situation and connexion with the neighbouring organs, it will be necessary to give a brief description of the chest, and of its other contents.

The thorax or chest, denominates the upper part of the trunk of the body. The structure of the thorax is chiefly of bone; the ribs and intercostal muscles form its lateral defences; and the breast bone and spine, to which the ribs are connected by cartilages and articulations, guard it before and behind. This bony circumvallation is widest at the bottom, and becomes gradually and pretty uniformly narrower as it appproaches the top. It is, therefore, properly said to resemble a cone. Strong muscles and ligaments, passing across from the upper ribs to the cervical vertebræ and to the head, close the contracted apex, leaving only a passage for the trachea, œsophagus and blood vessels, which are all at their entrance into the chest securely attached

on every side to the surrounding structure. The floor of this cavity is formed by the diaphragm, a broad muscle which bisects the trunk of the body transversely and separates the chest from the abdomen or belly. The œsophagus again passes out of the chest on its way to the upper orifice of the stomach through the diaphragm, which is also penetrated by two large venous and arterial trunks. Both the œsophagus and the blood vessels are on all sides firmly bound to the diaphragm in their passage through it. The cavity of the thorax, therefore, is every where securely inclosed and is pervious only through the bores of the blood vessels and other tubes which enter it at its apex and base.

The structure of the whole of the diaphragm is muscular, excepting a circular portion in the middle, extending to the anterior edge, which is tendinous. In the living and sound body, and after death, except when that has been occasioned by peculiar diseases, the diaphragm swells up a considerable way into the chest, exhibiting a great convexity on the upper surface and a corresponding concavity on the side facing the abdomen.

On the upper surface of the diaphragm and exactly on the middle and anterior part, where

the structure is tendinous, is placed the heart.—This organ has been already fully described. It is inclosed in a strong tendinous undilatable bag, called by anatomists the pericardium. The surface of the heart is every where free and unattached ; but the pericardium is connected to the tendon of the diaphragm on which it rests by a broad and strong adhesion. The trunks of the large blood vessels, in proceeding from the fundus of the heart, penetrate the pericardium, become firmly bound to it all around, and receive a covering from the extension of its internal membrane. As no substance intervenes between the pericardium and the heart, that envelope necessarily assumes the form of this organ.

The other contents of the chest are the lungs, the thymus gland which is almost obliterated in the adult, the œsophagus or gullet to which it affords a passage from the throat to the stomach, blood vessels, nerves and some membranes.

Of these, next, and perhaps equal, to the heart in importance are the lungs. It will be necessary to describe the situation and fabric of the lungs with more minuteness than may appear at first view to be demanded, in an inquiry confessedly limited to the motion of the blood.

The wind-pipe or trachea, immediately after it has passed from the neck into the confines of the chest, divides into two branches; one of which pursues a direction to the right, and the other to the left side. Each of these branches subdivides into a number of smaller branches, which again ramify, and again become stems of fresh ramifications; till at last they become extremely minute and multiplied. The ramifications of the divisions of the trachea are called bronchia or bronchial vessels; they are also called air vessels. The first ramifications of the bronchial vessels continue for some time to preserve the structure of their original trunk, the wind-pipe; they are formed by a succession of cartilaginous rings, which are connected to each other by strong, elastic, membraneous tubes, and which are so constituted that the lower ring will pass into the circumference of that immediately above it. The rings are not cartilaginous all around, but at one part of the circle are composed of elastic membrane, and may easily, therefore, become of a larger or less diameter. The ramifications of the bronchia, at length, as they become minute, drop their cartilaginous structure altogether and become solely membraneous, and terminate in cells. These cells generally form clusters which are

called lobules, and which each belong to some division of the bronchia. Inflation proves that the communication between the cells of each lobule is free, but that between the cells of one lobule and those of another difficult, though not impracticable. A cellular substance occupies the interstices between the bronchia and cells, and surrounds each of the lobules.

The two original divisions of the trachea form, with their ramifications and connecting substance, distinct and separate lungs, called from their situation right and left, or right and left lobes of the lungs. Each of these lobes is surrounded with a cellular substance, and, as has been remarked, is inclosed in a duplicature of the pleura, the membrane which lines the internal cavity of the chest. The two lobes of the lungs are, besides, divided from each other by another folding of the pleura, which is called the mediastinum, and which is placed vertically in the middle of the chest between the breast bone and the spine. The two laminæ or plates of which this particular folding of the pleura is formed, are connected at their insertions into the spine and breast bone, but in the middle and towards the bottom they separate and inclose the pericardium and its con-

tents. This inclosure is formed chiefly by the diverging of the left plate of the mediastinum. There are, therefore, in the chest three distinct compartments, one on each side of the mediastinum, containing the right and left lobes of the lungs, and the third in the middle forming a space for the heart. The lungs, therefore, are separated from the pericardium only by a single plating of the mediastinum, a thin, transparent, flexible membrane, easily dilatable, scarcely elastic, and on the left side, especially, lying loose upon the pericardium and surrounding it like a shield. As far, therefore, as the affording, on one side or the other, any support against pressure, or defence against injury, may be considered, the lamina of the mediastinum in contact with the pericardium may be regarded as nothing.

The shape of the lungs is not unfitly compared to that of the foot of an ox reversed. They are firmly bound to the top of the chest, but are at all other parts free and unattached. As no substance intervenes between the external surface of the lungs, and the walls of the thoracic cavity, they may be said to be suspended in vacuo.— They are exactly adapted to all the inequalities,

and to the constantly varying dimensions of this cavity, by the weight of the atmosphere pressing upon the internal surface of the bronchia through the trachea.

The substance of the lungs is powerfully elastic. This is admitted by all anatomists. If a piece of the substance of the lungs be taken while its structure is still perfect, and stretched into double or treble its ordinary length, it will, upon the removal of the distending force, resiliate briskly into its former dimensions. This property was confidently to have been presumed from the cartilaginous and ligamentous structure of the bronchial vessels.

From the moment that the first inspiration is drawn by the infant to the close of life, and after it, so long as the structure of the frame is uninjured, the lungs are swelled into a forced or unnatural state of expansion; that is, they are distended into dimensions far more ample than those which they would assume, if all force or restraint was withdrawn from them. The power by which they are kept in a constant state of forced dilatation, is unquestionably, what has been stated, the weight of the atmosphere.

As it is of the utmost importance in this inquiry, that the opinion now advanced respecting the ordinary condition of the lungs, should be clearly and satisfactorily proved, it will be necessary to enter fully into the arguments by which it may be supported.

The lungs at or soon after birth, in consequence of the increase of the thoracic cavity, occasioned by the depression of the diaphragm and other changes which take place, are dilated into much larger dimensions than those which were assigned them before birth. It was to have been presumed from the structure of the lungs, that this augmentation of bulk would be accompanied by a stretching of the elastic substance beyond its original, and in all probability, natural state.

This presumption is fully confirmed by experience. If an opening be made into the cavity of the chest, through any part of the walls, the lungs immediately collapse, and return nearly to the dimensions which they would have occupied before birth; only making allowance for their actual increase by growth of substance, which may be supposed to correspond with the difference between the bulk of the body at the

time the opening may be made, and that of the unborn fœtus. By such an opening, both the external and internal surface of the bronchial vessels, are equally subjected in every position to the pressure of the atmosphere; and the lungs have, therefore, full freedom to assume the form and dimensions which their structure may have made natural to them.

The collapsing of the lungs is, in this experiment, generally imputed to the admission of the weight of the atmosphere upon the external surface of these organs; and this is certainly true. But unless there were some other cause, the effect could not be produced. For the pressure of the air is equal both internally and externally. The specific gravity of the substance of the lungs, may, in some conditions of the body, have a certain share in the production of the phenomenon, but it must evidently be very small. Indeed the collapse is equally complete when the chest is erect, and when the specific gravity of the substance of the lungs must favour the distention of those organs.

In order to ascertain the strength of the collapsing effort of the lungs, the following experiments were devised.

On the 30th of March, 1815, the wind-pipe of a bullock which had been newly slaughtered, was laid bare from the larynx to its entrance into the chest. It was divided transversely near the top, and dissected from the other parts as far as the chest. The bullock was placed upon his back, with his shoulders higher than his buttocks. The wind-pipe was then bent into a pitcher full of water, so that the top of it reached ten inches below the surface of the water. An opening was then made into each side of the chest to admit the air. The sound of the air passing through the orifices into the chest, was distinctly heard for some seconds, without the surface of the water in the pitcher being in the least agitated. In my impatience I raised the wind-pipe about two inches, when bubbles of air issued from the surface of the water, and in a few seconds subsided, after which, another strong ebullition prevailed for a few seconds more.

On the 4th of the succeeding April, the wind-pipe of a sheep, which had been just killed, was treated exactly in the same manner with that of the ox, in the above experiment. The sheep was supported on his hind-quarters, with the chest in an erect posture. A piece of lead was tied to

the end of the wind-pipe, and sunk in a pitcher full of water. The wind-pipe was then fixed in the water, so, that its top was immersed under the surface of the water, to the depth of seven inches. Two orifices were then made into the chest, one on each side. After the air had been distinctly heard issuing through the openings for about 20 seconds, a bubble of air was observed to escape from the surface of the water in the pitcher, into which the top of the wind-pipe had been sunk, and in about four seconds more another rose. As the sheep was very fat, and the orifices small, the air did not enter into the chest freely. The openings were enlarged, when immediately the surface of the water in the pitcher was violently agitated for a few seconds, as if it had been boiling strongly.

On the 12th of the same month another sheep, newly slaughtered, was treated in the same manner, only that the end of the divided wind-pipe, instead of being inserted into a pitcher of water, was firmly tied so as to prevent the escape of any air from the lungs. Openings were then made into the chest; the lungs collapsed briskly as in the other instance, but did not recede within their lowest collapsing dimensions; for after some time

the ligature was removed; air escaped; and the lungs gradually collapsed still further.

These experiments decidedly prove that, in any situation in which the fabric has not been injured by disease or violence, the lungs are always in a state of forced expansion; that, when relieved from the agency of those powers by which they were expanded, they recede into their natural and comparatively narrow limits, in opposition to no inconsiderable resistances; and that they evidently derive the power of contraction, as it is retained long after the extinction of life, from a property inherent in their structure.

The weight of a column of water of seven inches in height, which the contractile power of the lungs balanced in these experiments, though it may be considered as valuing the elastic force as it exists in the lungs towards the termination of its long range, is not by any means to be taken as the measure of the momentum with which the lungs would resiliate at the stages of fuller dilatation and particularly at the stage of full inspiration. For the powers which are required to stretch any elastic substance must bear some proportion to the extent to which that substance

is to be stretched. Greater force would no doubt be necessary to stretch such a substance to treble than to double its ordinary length, and the momentum, with which it resiliates, would bear, at the different stages of its extension, a certain proportion to the multiple of its original length at those stages. The measurement of the resisting force, which the lungs possess while the walls by which they are inclosed have been uninjured, is not ascertained by these experiments; for the air upon which this force acted, is a fluid easily compressible; and, before such a degree of elasticity, as would support a column of water a few inches in height, could be given to the air in the lungs by compression, the volume of this fluid must have been greatly diminished; the dimensions of the lungs in consequence proportionably decreased, and the intensity of the elastic force greatly reduced. These experiments, therefore, do not afford any measurement of the force with which the lungs collapse at the commencement of that action, or, in other words, the proportion of the weight of the atmosphere which is employed in dilating them to the state in which they exist in life; but, from the degree of elastic power shown by these experiments to be possessed by

the lungs towards the termination of the long range of that elasticity we may infer its very high amount at the utmost extent of that range.

It is evident, therefore, that there is a constant and powerful tendency in the lungs to collapse and retire from the internal surface of the thorax, that this effort to separate from the confines of the chest will be measurable by the share of atmospherical pressure which the elastic power of the lungs is at the instant capable of supporting; but as this power is by no means equal to the whole weight of the atmosphere, and as no elastic substance intervenes between the lungs and the cavity of the thorax, no actual separation can be produced.

It is further evident, that all the parts which compose the walls or boundaries of the chest, are pressed, or, in common language, drawn towards the spaces from which the lungs have a tendency to retire, with a force which is commensurate with the elastic power of these organs, or with the share of atmospheric weight which that power is at the instant capable of supporting. The bony structure of the thorax is unyielding;

the diaphragm * is forced to assume a tense convexity towards the centre of pressure.

In what condition is the heart in these circumstances? It must necessarily be considered as constituting a part of the walls or boundaries of the chest; for there is a free communication between its internal cavities and the parts without the thorax, as well as the substance of the lungs, particularly with the mass of fluids through the bores of the large venous trunks. Between the external or convex surface of the heart and the surface of the lungs, membranes alone intervene. The divided laminae of the mediastinum are thin, pliant, easily dilatable, and evidently incapable of supporting, either on the upper or lower surface, any share of preponderating pressure. The pericardium, indeed, which surrounds the heart, and which is every where in contact with it, is a strong, firm, inelastic bag, and cannot be expanded beyond certain limits, without such a powerful force as would at the same time injure its structure, but of itself it will not resist any pressure that may rest upon its external surface. It is fitted to resist with ease the balance of unequal pressure,

* See Appendix.

which may be laid upon its internal or concave surface by the collapsing effort of the lungs.—But before it can be brought to support any share of this balance it must be dilated by its contents to the utmost extent. Until the pericardium has been extended to the extreme limits of its expansibility, the unequal balance which the boundaries of the chest have every where to support, must be resisted at this part by the walls of the heart. A force equal to the weight which the elasticity of the lungs, in their ordinary condition during life, is capable of supporting, must constantly tend to dilate the heart to the utmost limits of the pericardium, by ponderating against the internal surfaces of its chambers; and until these chambers are expanded in such a degree as would swell the general dimensions of the heart to an equality with the capacity of the pericardium, this force must all be sustained by the walls of the heart itself. As the pericardium cannot be distended further, through any other means than the expansion of the chambers of the heart, and as a full dilatation of the greater part at least of those chambers would be required to give to the heart dimensions commensurate with the utmost limits of the cavity which the pericardium is capable of inclosing; the successive

dilatation of the different chambers of the heart after contraction is fully secured by an adequate power.

By removing, therefore, in consequence of their elasticity, a part of the pressure of the atmosphere from the convex surface of the heart, and by that means causing the ordinary pressure to ponderate unequally against the concave surfaces of the chambers, the lungs become the certain and powerful antagonists of the muscles of the heart. The contractile force of the heart is necessarily more powerful than the antagonist action derived from the elasticity of the lungs, but the former is transient and interrupted, while the latter is equal and permanent; during the intervals between the exertion of the muscular energy, it becomes predominant; and, co-operating with the natural tendency of the structure, restores the chambers of the heart to the state from which they had been forced, and at which the superior strength of the contractile power begins again to be exerted.

Is that balance of weight, which is to be sustained by the whole external surface of the walls of the chest, in consequence of the resis-

tence removed from the internal surface of these walls by the resilient effort of the lungs, to be supported at this part by the walls of the heart itself, or intermediately by the pericardium?

Before we proceed to inquire into the answer which it may be possible to give to this question, it is necessary to make some explanation of the language we are obliged to use in the statement of it. The heart has been said to constitute a part of the walls of the chest. What is termed, in relation to the above question, the internal surface of the part of the walls of the thorax formed by the heart, is external with respect to the heart itself; the internal surface of the chambers of the heart must be considered as external with respect to the thoracic cavity.—To avoid anbiguity, the epithets concave and convex will generally be applied to distinguish the surfaces of the heart.

The degree to which the pericardium is dilated during life, and the dimensions, which the heart occupies, compared with the extent to which the pericardium is dilatable, have not hitherto been determined by observation. For no observer has ever been so fortunate as to see the heart and

pericardium in the condition in which they exist in the living or sound system. Whatever admits, into the regions which those organs occupy, the vision or the touch, must have already admitted an influence destructive of the machinery. The springs of life must have been broken; the invisible chain by which the heart and the lungs are connected must have been loosened; these organs must have sunk from their condition and their place; and, in the scattered fragments, we shall search for the forms of their pristine existence, with no better success, than the school-boy examines the few drops of spray for the fabric of the beautiful bubble that lately danced in the air, or inspects the cavity of the drum into which he had cut for the purpose of discovering the origin of its sound.

If the balance of weight, ponderating at all times against the external surface of the boundaries of the chest, is to be sustained at one part by the pericardium; then this envelope will be at all times fully dilated; and the heart will always occupy the same dimensions; dimensions commensurate with the cavity of the pericardium dilated to its utmost capacity. But if the weight is to be sustained at this part by the walls of the

heart, then the pericardium will rest loose and easy upon the convex surface of this organ, to the varying bulk of which no fixed limits will be assignable.

Whether in general, the inequality of pressure, arising from the causes that have been explained, be ultimately sustained by the pericardium or by the walls of the heart, is a question which, I fear, cannot, in the present state of our information, be certainly answered. But it is contended, that in the statement of one of those two suppositions, the true and natural condition of these parts must be accurately described. As the actions of the heart must, upon the first of these suppositions, in particular, being true, be modified by the pericardium in an important manner, it will be necessary to consider those actions under both suppositions; and, by comparing the cases, we may be enabled perhaps to form a plausible conjecture, at least, of the true condition of these parts during life.

In the first place, let it be supposed that the preponderance of atmospheric pressure is sustained throughout all the movements of this organ, by the walls of the heart itself.

According to this statement of the case, it is evident, that the concave and convex surfaces of the heart must be at all times unequally pressed; that the balance of this unequal pressure must rest upon the concave surface of these walls; and that this balance is exactly equivalent to the elastic power of the lungs. Unless there had existed in the heart itself an antagonist and recovering power, it is evident that all the chambers of the heart must, in these circumstances, have been in a state of forced dilatation. The manner, in which the constant and uniform operations of unequal pressure are counteracted by the stronger but interrupted exertions of muscularity, has been already described.

As the ventricles are much thicker than the auricles, a greater power might be supposed to be employed in dilating the former than the latter, whereas, according to this view, it is equal. But the difference in dilatability, in the different parts of the heart, may be only apparent; and that inertness, supposed in the ventricles to arise from massiness of structure, may be more than counterbalanced by the figure and position of the muscular fibres; as, in consequence of these, a strong expansive effort, which must at all times

be in proportion to the massiness of the structure, necessarily accompanies the simple relaxation of those fibres. Some aid may possibly be supposed to be contributed to the expansion of the ventricles by the projectile power of the auricles which are at this moment resuming their systole; although the counteracting the resilient power of the lungs may be conceived to be at least as great a labour as these chambers are able to accomplish.

Whatever other difficulties may appear, in this view of the case, it must be allowed that, according to it, the full expansion of the chambers of the heart are abundantly secured.

Let us next consider the other supposition on this point, that the pericardium is always distended by its contents to its utmost limits, and that, therefore, the heart in the living system always occupies the same dimensions.

This supposition is not new. It has been supported by many able physiologists, particularly by the celebrated Lieutaud. The heart may easily be supposed to be at all times of the same bulk. As the conditions of the different

parts vary, the dimensions of the whole may remain the same. When the ventricles are in the diastole, the auricles are in the systole; when the auricles are in the diastole, the ventricles are in the systole; and, at any intermediate point, the ventricles and auricles are in reciprocal proportion, with respect to their degree of dilatation. It is evident that in all these situations of the heart, the same quantity of blood, by which the difference in its bulk can alone be produced, may be contained by it.

Suppose, in this case, the ventricles to be in the diastole and about to contract. A quantity of blood is thrown by this action out of the confines of the pericardium. To preserve the equilibrium of pressure between the contents of the pericardium and the substances without this envelope, the capacity of the pericardium will necessarily either be diminished to an extent that will be commensurate with the bulk of fluid, that has been expelled by the ventricles, or, the same bulk of fresh matter must be introduced into this cavity. From the convex surface of the pericardium, a share of the weight of the atmosphere is removed by the collapsing effort of the lungs. But the blood in the arteries and veins,

with which the cavities of the heart have a communication, is subject to the usual pressure.—Valves and the condition of the ventricles at the moment prevent the return of blood from the arterial trunks. But the entrance of the venous blood is not opposed by valves, and is favoured by the condition of the auricles at the time.—The quantity of matter, therefore, required to occupy the space which the expulsion of blood from the ventricles would leave, is necessarily supplied from the veins.

But the ventricle again becomes relaxed; and, in consequence, expansion to a certain extent takes place. Synchronous with the natural expanding nisus of the ventricle is the resilient or contracting effort of the auricle. The blood, therefore, while it is pressed upon by the contracting auricle, sustains a diminished resistance on the side of the ventricle, and necessarily flows all into that chamber. Proportionably as the auricle contracts, or is diminished, is the ventricle less resisted on its convex surface, while its concave surface is pressed with additional force; and thus the full dilatation of that chamber is secured.

During this movement, no blood is either received into the confines of the pericardium, or expelled out of them, but a quantity is only transferred from one part of these confines to another.

According to the last view of the condition of the pericardium and heart, the movements of this organ are evidently performed with greater ease, and the labour which each part has to perform is more adapted to its apparent powers. There is in this, as in the preceding supposition, an evident necessity for the massy structure and great strength of the ventricles; as their office is not only to propel a quantity of blood with great force into the arteries, but also, in addition, to overcome the antagonist power of the lungs. But there is a point at which the ventricles may be conceived to have thrown off their load and to be at rest. At the period of full dilatation, the force by which the ventricles are dilated is removed from their concave surface and supported entirely by that of the pericardium. An opportunity may thus be conceived to be afforded this organ for recovering itself.

Upon the last supposition, the aid that is afforded to the auricles during their movements

is remarkable. The auricles, while contracting, are not required to overcome the collapsing power of the lungs, which at this period is removed from all parts of the heart, and supported wholly by the pericardium. As the equilibrium of pressure is perfectly adjusted between the contents of the pericardium and the parts without this envelope, during the contraction of the auricle, the whole power of this chamber is reserved for the dilatation of the ventricle. Nor is this power required to be considerable. For the simple relaxation of the muscular fibres of the ventricles, so far as it tends to dilate them, must, in these circumstances, act with a double effect. As the ventricular cavity is enlarged, the blood is drawn from the auricle; and, in consequence of the diminution of the auricle necessarily following, the pressure on the convex surface of the ventricle is diminished. Indeed, admitting that the muscular fibres of the heart could, by their relaxation simply, in circumstances the most favourable, resume the form and position that are natural to them, it is not necessary to consider the auricles as more than inactive receptacles provided for the convenience of the ventricles; as, in that case, the depletion and impletion of the former chambers would be the necessary

effect of the actions of the latter. At all events the positive power of the auricles, whatever it may be, must in these circumstances have a double influence. For, by their projectile power, on the one hand, the pressure upon the concave surface of the ventricles is increased; while, on the other, by the diminution of their bulk necessarily accompanying that projection, the resistance on the convex surface of the ventricles is lessened.

The full dilatation of the ventricles would be, in thisview of the case, perfectly secured, without the necessity of ascribing to the auricles a labour apparently incompatible with their structure; the alternate movements of the ventricles and auricles would be regulated with unerring certainty; and the salient movements of the heart would be well explained from the changes of figure and position induced in that organ, especially during the contraction of the ventricles.

It may be objected against this suppositon, that a greater quantity of blood is propelled from the ventricles, by a single contraction, at one time than at another. But this may arise from the contractions being more complete at one time than at another. It is evident from the

structure of the ventricles, that they can never, in any state of that organ, be entirely emptied.—There must at all times be a quantity of blood in them. But they may be more nearly emptied by one contraction than by another. Thus a greater quantity of blood may be circulated through the heart, by a given number of contractions, without any alteration in the general dimensions of the heart, or without its containing at any one instant a greater quantity of blood.

Although the pericardium may not generally be fully distended by its contents, but lie loose upon them, there can be no doubt, that, if at any time, by the accession of a greater quantity of blood, or by an enlargement of the solid parts, it should be completely filled, this envelope would then serve to regulate the motions of the heart, according to the manner just explained. Security is thus afforded by the pericardium to the ventricles and auricles against the danger of their being overcome and kept in a state of distension by the antagonist influence of the lungs.

The degree to which the pericardium is generally distended by the heart and its appendages, cannot be correctly established; but the argument,

so far as it may be strengthened by a more exact correspondence of the actions with the means employed and the ends in view, seems to preponderate greatly in favour of the opinion, that the pericardium is always fully expanded by its contents, and that the heart is at all times of the same dimensions.

But whatever may be the extent to which the pericardium is generally expanded in the living system, the dilatation of the chambers of the heart, upon the supposition of its arising from the causes to which it has been ascribed in this inquiry, must produce the same effect upon the motion of the blood, both in its passage through the heart itself, and in the venous system generally.

In consequence of the pericardium being all around, except at its inferior flat surface, where it is attached to the diaphragm, invested by the lungs, and of the tendency which these organs must have of receding from the surfaces, in contact with them in the direction of the perpendicular to these surfaces; the direction of the force derived from the resiliency of the lungs, as it affects the pericardium and walls of the heart, must be divergent. Hence the uniform expan-

sion of these walls is secured. If the decrease of resistance had been sustained in one direction, only, or if the walls of the heart had been pressed to one central point, however much the fibres may have been stretched, the chambers of the organ could not have been dilated, by that power.

The manner, in which the blood immediately in the chambers of the heart will be affected by the movements of this organ, must already have appeared abundantly evident; let us next inquire, how the blood in the venous system generally will be influenced by them?

The evident and incontrovertible consequence of the abstraction of a certain portion of the general weight of the atmosphere, from the convex or outward superficies of the pericardium, by the resiliency of the lungs, and of course from the convex surface of the auricles, either directly, or intermediately by the contraction of the ventricles, is, that the blood, at the time in the large venous trunks in contact with the heart, must be less resisted on the side of the heart than at any other point; and that, in obedience to the laws by which fluids are governed, it must flow towards the centre of pressure, or into the auricles,

until these chambers have been dilated to an extent, at which their walls will afford a resistance equal to the resilient power of the lungs; or, until a quantity of blood is admitted into the boundaries of the pericardium equal to that which is projected out of them by the concomitant contraction of the ventricles. At every contraction of the ventricles, a quantity of blood is thus pumped out of the venous trunks into the heart. While any fluid is found in the roots of the veins in contact with the heart, a portion of it must by the suction powers of this organ, be successively emptied into the auricles, the recipient chambers of the heart.

The next question to be considered, is, How far the suction power of the heart would extend its influence through the venous system; and how the blood in the remote vessels would be affected by it? It is plain that the blood could not be raised by suction alone to the heart, through the veins, from parts at a distance from that organ, unless the intervening vessels were incompressible, or were so fabricated, that their tubular form would be preserved in opposition to a considerable balance of pressure ponderating against their external surface. But it is acknow-

ledged, that the coats of the veins are extremely thin, pliant, and yielding; and that they collapse in consequence of their own gravity alone. The bores of those veins, in which the advance of the blood is not favoured by gravity, would, by the supposed actions singly, be obliterated near the heart, and the blood, beyond the point of collapse would be left stationary, or become retrograde.

A portion of the motion of the venous blood is, unquestionably, derived from other causes.—If a vein is surrounded by a ligature, the part of the vein beyond the ligature swells and becomes tense; and, if an opening be made into it, blood will continue to flow with an impinging stream until the ligature is removed. All communication, between the swelled part of the vein and the heart, has been entirely intercepted by the ligature; and, in no circumstances, could the elevation of pressure from the blood on the side next the heart, cause any fluid to be projected out of the cavity of the vein. The momentum, indicated by the swelling of the vein behind the ligature, and by the projecting stream in venæsection, may be termed the positive momentum of the blood. Let us next investigate the sources from which this momentum may be derived.

It was stated in the preceeding part of this inquiry, that the blood in the extreme veins sustained an impulse from the arteries, successively as fresh blood continued to be thrown by the latter into the former; that the aggregate amount of the force of the contractions of the extreme arteries was estimated to be about equal to that of the heart; and that blood would be continued to be thrown into the veins, by this power alone, until its projectile force was balanced by the weight of the column of blood in the veins.

No data, it was stated, have been discovered from which the amount of the impulse, which the blood in the veins sustains from the arteries, can be calculated. If however we take into consideration the peculiar resistances which are opposed to the motion of the blood in the veins, from anastomoses which are particularly frequent in the small veins; from the converging dimensions of the venous system; and from the facility with which the coats of the veins are dilatable by lateral pressure, while no great share of such pressure is in ordinary circumstances discernible in these vessels, it may be concluded that the effects of the *vis a tergo* would not singly extend far into the venous system. It is admitted in-

deed that the momentum of fluids may be continued, either by apportioning the impelling power to the motion required while the resistances remain the same; by diminishing the resistances without increasing the impulse; or by a combination of those causes. The venous blood may now be said to be influenced in both ways. While the impulse is constantly renewed from behind, the resistance is diminished in front, and consequently the whole mass may be advanced. But the distance, between the blood which sustains the supposed impulse and that from which the pressure is originally removed, is, in many instances, so great; and the intervening vehicles so pliant and yielding, that it is impossible to conceive that these causes could operate in conjunction. Extensive intervals would be left in which the blood would stagnate or its motion be reversed; especially if gravity, from the position of the body at the time, should oppose the reflux of the blood to the heart.

Besides, there are certain veins in which the blood cannot be conceived to be affected by a *vis a tergo*, and in which the positive momentum is nevertheless found to exist, in the same extent as in all other parts of the system. If a

ligature be put round the internal jugulars which collect the blood from the sinuses of the brain, the part of the vein beyond the ligature will swell, even when the position of the head is dependent. It will scarcely be contended that any impulse could extend from the arteries of the brain to the internal jugulars, when it is considered that this impulse must be conveyed through the veins of the brain to the sinuses and through them to the jugulars. Such a supposition must be made in defiance of all probability.

Gravity has been alluded to as a power, influencing, in certain circumstances, the motion of the blood. It is not a little surprising that the effects of this power are not more visible in the venous circulation. A vein in the leg, for instance, in the sound frame, is scarcely observed to swell more, or become more tense, in the erect posture, than the same vein in the recumbent state of the body. This phenomenon is unsatisfactorily explained by the doctrine that the weight of the tall column is sustained at different heights by valves.

To what extent may gravity be rendered available to the circulation in the venous system generally? Of this question it is feared that we

are not prepared to give a full and satisfactory solution. The following observations may, perhaps, throw some additional light upon the subject.

The plain and universally admitted principle, upon which gravity influences the motion of fluids in tubes, is, that if a fluid of the same specific gravity throughout be placed in vessels between which there is a free communication, it ascends or descends, until the fluid in all the vessels becomes of the same level. Capillary attraction presents the only exception to this rule.

When treating of the structure and arrangement of the blood vessels, it was remarked, that anastomoses or vascular communications were to be found, not only between the branches which had arisen from the same trunk, but between those belonging to different trunks; and that these communications prevailed, particularly and to a great extent, in the venous system. The veins, therefore, which constitute a large proportion of the animal fabric, may be considered as ramifications of particular trunks, that these ramifications, in their progress, form an irregular net work of tubes, communicating with each other, in such a way, that a fluid may readily pass from one tube

into any other, without returning to the original stem from which there tubular ramifications arose.

In this view of the subject, the blood in the vena cava and its divisions may be supposed to be placed, with respect to that in its smaller ramifications, between which anastomoses generally prevail, and which on that account may be deemed a single vessel, in such a manner as to constitute an opposing or balancing column. As the fluid is in both of the same specific gravity; the height of it in the one column will, unquestionably, be regulated by its height in the other. Let it be supposed then, that the system of the vena cava has been filled with blood, and that this blood has subsided in all its parts to an adjusted equilibrium, and let this system, in the state supposed, be subjected to the action of the heart and arteries, in the manner that has been described. By the stroke of the heart, a quantity of fluid is withdrawn from one end of the column, and by the synchronous vibrations of the arteries, an equal quantity is added to the other. While the adjusted level of the fluid is diminished on one side by the abstraction of a part, it is heightened on the other by the accession of fresh matter. A double derangement is the effect. By these

actions a motion must be excited in the fluid generally to restore the equilibrium between the different parts of it. But the causes are perpetually renewed; therefore a perpetually repeated generation of motion must be produced through the different parts of the venous system by gravity, and this motion must be from the ends of the veins to the trunks.

It is immaterial from what part of the column in relation to height, the portion of fluid be abstracted; or to what part the addition be made; the derangement of equiponderance in the fluid generally, will evidently in every case be the same,

It appears then, that, in this manner, gravity augments the impulse which the venous blood sustains from the heart and arteries, and, in all probability, produces a great proportion of that which has been denominated the positive motion of the blood in the veins, and which is indicated by the swelling observable in the part of the vein beyond the ligature.

The force communicated to the venous blood by the heart and arteries, assisted by gravity and favoured by the natural structure of the vessels,

may be supposed to be sufficient to preserve any part of the venous system from a permanent state of collapse; and thus, as the actions of the heart are renewed, to place a fresh supply of blood within the sphere of its influence. The blood, thus solicited and impelled, continues to flow, in an uniform continued stream, from the ends of the veins to the heart.

We are now enabled to discern the necessity that exists for the structure of the veins being different from that of the arteries. We have already seen how well the arteries were fitted for the performance of their peculiar offices. Had the veins been either elastic to any considerable degree, or muscular, the tendency of the powers derived from that structure would have been to diminish the cavity of the vessel. But such a tendency must evidently appear, from the above reasoning, to have been in opposition to the motion of the blood through the veins.

The smooth equable current of the venous blood, exhibiting no marks of sustaining any impulse from any other substance, or of acting upon any itself, not even producing the slightest agitation in the flexible tubes in which it flows,

through all the vicissitudes of its course, subject to rapid changes of direction, perpetually varying its elevation, and formed by apparently opposing streams, is a matter that is calculated to strike the mind with astonishment.

The motion of a fluid so rapid, but at the same time so imperceptible and quiet, as that of the blood in the veins, in vessels so flexible and easily distended, proves that no opposition can exist between the forces by which the different particles of blood are moved, but that every particle succeeds to the place of another with the same direction and force, that the preceding particle possessed at that place. But by what cause can such a description of motion be produced? Certainly not by any force acting partially upon the blood, whether we suppose the impulses to be made upon it at one end of the column, as by the *vis a tergo;* or laterally, by the pressure of the muscles, or the distension of concomitant arteries.

The description of motion, exhibited by the venous blood, seems to be deducible from the principles which we have attempted to establish. Every particle of blood sustains an equal share

of the pressure of the atmosphere. If this pressure, as is contended to be the case, be to a certain degree removed from any part of this body of fluid, the particles on one side of which the pressure has been diminished, will yield to the greater influence on the other side and advance; the next particles losing the usual support of the preceding and sustaining a greater weight on the opposite side, move into the place just left by their precursors, and in like manner the next until a motion is produced in the whole. The quantity of motion in the particles behind is regulated by that of those which preceeded. Hence, at any section of the current of venous blood, there is a constant change of the particles, but no change in their number, direction, or force. The motion of such a fluid may be deemed internal, it has limits assigned to it beyond which it cannot pass, nor affect any other substance. In the tendency of the blood to fill those limits, its motion in a great measure consists. The veins therefore can sustain no unequal lateral pressure, nor undergo any sudden alteration in length, from the fluid which is transmitted through them.

That the limits occupied by different portions of the venous system may be preserved the same,

at all times, in its natural state, it is necessary that a supply should be produced for every part equal exactly to its expenditure. The capillary arteries are well calculated to make this supply. These vessels communicate with the venous capillaries and pour their contents into them by regularly repeated discharges, at every distance from the heart. In consequence of the difference of distance, the times of the discharges also vary, or the last vibrations of all the arteries are not synchronous. Regularly repeated projections from an infinite number of orifices, varying with respect to the time and the distances with which those of each are made, but collectively pouring into the veins an equal quantity of fluid in equal times, will easily be conceived to produce uniform equal currents in the veins into which these discharges are made; and seasonably to apportion the requisite supplies. The anastomoses, which prevail so universally in the capillary veins, and which can so readily repair the deficiencies of one part from the surplus of another, must contribute greatly to the uniformity of motion in the venous circulation, and to the instantaneous supply of its demands.

A brief recapitulation of the causes by which the blood is supposed to be affected, combined

with a summary explication of the manner, and, so far as that can be ascertained, the proportion in which each contributes its share, may be necessary for the purpose of placing the whole theory of the circulation at once before the view of the reader.

By every contraction of the ventricles a quantity of blood is projected forcibly into the great arteries. As the coats of the arteries are dilatable, these vessels give way to the impetuosity of the propelled blood; and, to a certain distance from the heart, become of a more enlarged calibre. In consequence of the stimuli derived from the blood newly admitted into the arterial cavity, and (if so bold a metaphor may be used) from the pain of distension, the irritability of the muscular fibres is excited; the contractile effort is roused; and, co-operating with the elastic, restores the artery by a rapid movement to its former dimensions. As the blood is an incompressible fluid, a quantity equal to that projected from the heart, must now be displaced from that portion of the arterial system which had been dilated by the immediate impulse of the ventricles. The valves, which are stationed at the roots of the great arteries, and which are securely closed,

during the contraction of the first portion of the arterial system, by the retrograde movement of the blood nearest the heart, and more effectually by the suction occasioned by the synchronous dilatation of the ventricles, completely prevent the return of any fluid from the arteries into the heart; while the more ample calibre possessed by the aggregate of the vessels beyond the contracting portion, and the distensible condition of their relaxed fibres at the moment, favour its advance into a more distant portion. This portion, now distended and stimulated by the same causes by which the first had been excited, recoils vividly upon its contents; and, by the rapid resumption of its former capacity, expels from its cavity a quantity of blood equal to the addition which it had received during its dilatation.—The blood, displaced by this movement from what may be termed the second portion of the arterial system, is directed into a more advanced part of it, by the different state of the coats of the vessels and the difference between the aggregate of their calibre, before and behind the contracting portion; because, in consequence of the rapidity of the vibratory undulation, the coats of the vessels between the contracting portion and the heart are still rigid, while those beyond

it are relaxed and easily dilatable; and because the outlets on this side are greater, as the aggregate of the bores of the vessels increases with the distances from the heart. The same series of actions is repeated to the ends of the arterial system. A quantity of blood, therefore, equal to that which had been projected from the heart into the arteries, is propelled from the ends of the arteries into the veins in equal times. The power of the heart may thus be said to be transmitted undiminished to the end of the arterial system.

No data were discoverable from which the momentum of the blood, discharged from the ends of the arteries, or the quantity of motion it would generate in the venous blood, could be estimated. But from the mass of fluid that was to be put in motion; from the velocity with which it was known to flow; from the form and position of the vessels containing it; from the resistances opposed by friction, tenacity and various other causes; and from the phenomena attending the venous circulation; it was concluded, with the fullest confidence, that the blood could not be circulated through the veins by the impulse it received from the arteries alone.

In searching for the causes by which the chambers of the heart were dilated after contraction; it was ascertained, that this condition of the organ was in part to be ascribed to the form and position of its fibres in consequence of which simple relaxation was accompanied by a certain degree of dilatation; but particularly to the supporting of a part of the atmospherical pressure that would have rested upon the convex surfaces of the heart or its envelope, by the resilient or collapsing effort of the lungs. It was urged, that the abstraction of a part of the ordinary pressure of the atmosphere, from the convex or external surface of the heart or from the convex surface of the pericardium, was perpetual, and was therefore always ready to co-operate with the dilating faculty of the heart itself as that was alternately renewed; and that the conjunction of these powers was fitted during the intervals of contraction to dilate the chambers, to the utmost extent or at least to the extent of the capacity of the pericardium.

In consequence of the dilatation of the ventricles by the causes which have just been stated, the valves at the roots of the arterial trunks, yielding to the greater pressure from without,

become securely closed, and the resumption of blood by the heart from the arteries is completely prevented; but the passage of blood from the auricular into the ventricular cavities is not obstructed; the blood, therefore, by which the former chambers were dilated, pursues the less resisted course, and occupies the space left by the dilating ventricles. Any deficiency in the full dilatation of the ventricles, which in the healthy condition of these parts can scarcely occur, will readily be supplied by the projectile force of the contracting auricles. By the dilatation of the auricles, the valves, in the auricular passages, sustaining less resistance on their internal surface, become securely closed; but, at the other openings, those by which they communicate with the venous trunks, no obstacles are interposed. The blood, therefore, in the large venous trunks, is relieved from a part of the ordinary pressure in the direction of the heart; it necessarily takes the course in which it meets with the least resistance, and continues to move in that course until the resistence is equalised by the full dilatation of the auricles.

The heart, therefore, acts at once in a twofold capacity. By the contraction of the ventricles it

propels the blood through the arteries, and by the dilatation of the auricles it pumps it from the veins. It is at the same time a forcing and a suction pump.

The structure of the veins is not fitted for raising blood through them to the heart, from parts at a distance from that organ, by suction alone. For these vessels, being very thin and pliant, would immediately collapse and become impervious under such an influence. Other agents are required to preserve the permeability of the venous vessels, to give them as it were the property of rigid tubes, and to bring the blood which they contain generally within the sphere of the action of the heart. For this purpose, two causes are supposed to co-operate. The first, to which allusion has already been made in this recapitulation, is derived from the projectile power of the ventricles, transmitted by the vibrations of the arteries to the ends of the veins. This is the *vis a tergo*, so famous in the schools of medicine. We were unable to estimate the share of the venous circulation that is to be attributed to this cause; which, however, so far as it extends, is evidently well fitted to co-operate with an abstraction of resistance in front, and to

preserve uninterrupted the communication between the blood in the remote parts and the heart.

The other power, which was supposed to assist in preserving the distension of the venous vessels in opposition to the suction influence of the heart, was gravity. By the anastomoses which so generally prevail among the veins, particularly among the smaller ramifications, the system of the vena cava may be considered as a single canal. By its retiform fabric, the communication, between the blood in the different branches by the aggregate of which any portion of this canal is formed, is preserved as ready and free, as if the blood had flowed in that portion in a single unpartitioned channel. The position of the vessel is fixed. At the moment in which the equilibrium between the contents of the vessel may be supposed to have been adjusted, a quantity of blood is abstracted from one end of it by the stroke of the heart, while a quantity is added to the other by the synchronous contraction of the ultimate portion of the arteries. The balance between the opposing columns of fluid is disturbed; to restore the equilibrium, deranged as the actions of the heart are renewed, a motion is

generated in the blood by gravity from the ends of the veins to their roots.

In short, the motion of the blood while it flows in the veins is produced by the force of the heart and arteries urging it behind; by the abstraction of a share of the atmospheric pressure from it in front, in consequence of the resiliency of the lungs, interposing its influence in the intervals between the contractions of the heart; and by gravity, which is rendered available in this case by the projection of the arteries and the diastole of the auricles.

PART III.

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The phenomena, which will not admit of a satisfactory explication from the operation of the causes hitherto assigned to the motion of the blood while it flows in the veins, but which are plainly deducible from the hypothesis attempted to be established in the preceding pages, are numerous and remarkable. It is hoped also that any difficulties, which at first view may oppose themselves against the plan of the venous circulation that has been proposed, will be removed upon a more mature consideration of the subject.

It must appear to every one, who has considered the circulation of the blood with any share of attention, extremely unaccountable, that the blood should flow with continued impetuosity from a divided artery, while the stream, from the accompanying vein in the same situation, is gentle and easily subdued; for, considering that the quantity of blood, which, in the sound state of these vessels passes through the vein, must be

equal to that which in the same time passes through the artery, and that the fountain from which each is supplied is the same, the venous stream ought to be equally strong and lasting with the arterial. Hence arose the unphilosophical conclusion, that the laws of hydrostatics were not applicable to the motion of the blood. But the surprising difference between the momentum and permanency of the stream from a divided artery, and of that from a divided vein, admits, upon the principles we have attempted to establish, of a solution, at once simple, satisfactory, and perfectly conformable to the laws of hydrostatics. It has been maintained that an important share of the motion of the venous blood is derived from the diminution of resistance in the direction of the heart, occasioned by the removal of a part of the atmospheric pressure, from the convex surface of that organ, by the elastic effort of the lungs. But when a vein is divided, the blood contained in the part of the vein beyond the point of section, is, on all sides, exposed to the undiminished weight of the atmosphere, and a powerful cause of its motion is, in consequence, withdrawn. The blood beyond the division is now placed in circumstances very different from those in which it flowed in the

sound vessel. If it be intended therefore to estimate, from the force with which the blood flows from a divided vein, the actual force with which it flowed before the division of the vessel, it will be necessary to add, to the discharging stream, a quantity of motion equal to that which would be generated by the share of atmospheric weight supportable by the elasticity of the lungs, at the full dilatation of these organs.

If a puncture be made in the side of an artery, the blood will continue to flow from the orifice, even to the extinction of life ; but if a similar puncture be made in a vein, no blood will be spilt or at least the discharge will soon cease.— According to the supposition, that the venous blood is propelled by a *vis a tergo*, and by lateral pressure, there appears no reason to infer, why the blood should not flow from the orifice made in the vein as permanently as from that in the artery. It would be pressed out of the orifice of the vein by a force measurable by the weight of a column of blood, of which the height is equal to the distance of this orifice from the heart, and the base to the area of the transverse section of the vein. The resistance which, in those circumstances, the blood would have to overcome in proceeding along the channel of the vein, would

unquestionably, be more powerful than that which would be opposed to it in flowing out of the orifice. The punctured vein ought therefore to bleed as freely, and as long, as a punctured artery.

We are now enabled to assign a satisfactory reason for a punctured vein refusing to bleed.—In taking its course out of the orifice, the blood would have to surmount the resistance which the whole weight of the atmosphere is capable of opposing; but in flowing along the channel of the vein it is relieved from a share of that resistance. It therefore necessarily takes the less resisted passage, and is as securely confined within the coats of the punctured, as within those of the uninjured, vein.

If a vein be surrounded by a ligature, the part of the vein between the ligature and the heart becomes empty and collapsed, while the part on the other side of the ligature swells and becomes tense. From the operation of the causes usually ascribed to the motion of the blood, the swelling of the vein, behind the ligature, will easily be deduced; but its depletion on the side of the ligature nearest the heart does not admit of so ready an explanation. The blood which had

just cleared the section of the vein about to be rendered impervious, would not, after the power, by which it is supposed to have been moved, had been intercepted by the ligature, continue impressed with such a degree of momentum as would be sufficient to convey it all to the heart, and evacuate the vein. According to the laws of fluids moving in tubes, part of the blood would, from the reaction of the particles, become retrograde and be driven back against the ligature. This would more certainly take place in tubes formed in the manner in which the veins are; for other branches unite with the stem from which the bound vein has sprung; the blood in those branches is not, as in this, intercepted from the influence of the powers by which it is supposed to have been propelled. The superior force of the streams, in the unbound branches, would necessarily prevent the blood, placed between their confluence with the bound branch and the ligature, and cut off from the influence of the propelling powers, from advancing into the common stem. But the part of the vein before the ligature, supposing it to have been by some means depleted, would soon be filled with a fresh supply, by a reflux from the other branches; and the coats of the vein, on the side of

the ligature nearest the heart, would continue to be distended by a force equal to that of the circulation in the veins. The retrograde motion of the blood could not in these circumstances be always at least prevented by valves.

The depletion of the vein before the ligature follows, necessarily, from the operation of those causes to which the motion of the venous blood has been attributed in the preceeding pages.—Yielding to the balance of pressure, which ponderates against it from behind, and which the application of the ligature could have no effect in lessening, the blood will continue to advance towards the heart after the permeability of the vein behind it has been destroyed; and the same causes, by which the part of the vein before the ligature was emptied, would effectually prevent it from becoming, in the same circumstances, the receptacle of a fresh supply.

If a ligature be put round all the branches or veins which return the blood from a particular part, as from the arm, so as to render them impervious, without at the same time compressing the accompanying arteries, the parts of the veins between the ligature and the extremity of the

member will swell and become very tense, and will continue so until the bandage shall have been removed, or until the death of the part. If a puncture, in these circumstances, be made in the swelled vein, the blood, without the relaxation of the bandage, will continue to flow even to the extinction of life. This appearance, with which mankind have been familiar from the most remote ages, was justly held by Dr. Harvey, to supply one of the strongest arguments in favour of the circulation of the blood; and, when we consider the inferences which it so plainly indicates, we are struck with surprise that it did not earlier direct the notions of mankind to that great discovery.

If, however, one of the veins only of the arm be compressed by a ligature, the part of the vein between the ligature and the extremity of the member will swell, but not to the same extent, nor with the same continuance, as it would have done had all the veins of the arm been compressed at the same time. Nor will much blood in general be discharged from an orifice made in the vein behind the ligature. The uncertainty of a vein always swelling behind the ligature with which it is surrounded, and the speedy dis-

appearance in many instances of the swelling after it had taken place, are circumstances which gave great perplexity to the first supporters of the circulation of the blood, which supplied their keen adversaries with powerful weapons, and which it must be acknowledged do not admit of a satisfactory explication from the views that have been afforded of the causes of the Harvean circulation. The uncommon interest which the circulation of the blood created at its first promulgation, in consequence of the violence with which it was assailed by its ardent opponents as well as of the vast importance of the subject, and the plausibility of the objection afforded by the absence or speedy cessation of tumor behind the ligature by which a vein had been tied, caused these experiments to be multiplied exceedingly, and to be repeated on all kinds of animals, in every variety of form.

The illustrious Baron Haller, unquestionably one of the ablest and most zealous followers of Harvey, with that candour, for which that great physiologist was eminently distinguished, admits indeed the difficulty, but at the same time attempts to elude it by devices that are more dexterous than convincing. He advises the experi-

menter in these cases to be quick in making his observations; as otherwise the swelling behind the ligature may have in the mean time subsided, in consequence of the failure of propelling force accompanying a cessation of the powers of life. He also recommends as the subject of these experiments warm blooded animals rather than those which are less perfect; observing, that in the latter the blood soon retires from behind the ligature. A remarkable circumstance this; for if the decrease of the swelling proceeds from the sinking of the powers of life, the swelling behind the ligature ought in such experiments to continue longest in the cold blooded animals, as they are more tenacious of life than those which are termed the more perfect. The opponents of the Harvean theory maintained, that the appearances observed upon tying a single venous ramification, tended most powerfully to confirm the opinions which prevailed respecting the motion of the blood, before the doctrine of the circulation had been proposed; and which were, that the blood flowed to and from the heart alternately in the same vessels, after the manner of the flowing and ebbing of the sea. The retiring of the blood from behind the ligature was supposed to occur during the refluent movement of the fluid. Mi-

croscopic observations made upon the blood flowing in the transparent parts of animals, added to the perplexity into which the supporters of the Harvean theory were thrown. Globules of blood were observed, after advancing through a vein, and trying unsuccessfully, as it were, to find a passage onwards, to retrace their steps, and to retreat, for a considerable space, along the same channels in which they had before advanced.

All these appearances will derive a plain and satisfactory explication from the supposed operation of the causes to which the motion of the venous blood has been attributed, and the objections which these appearances were supposed to oppose against the doctrine of the circulation, will be completely removed.

When all the veins, belonging to the arm, are compressed by ligatures or by a bandage, every avenue for the return of the blood from the part beyond the ligatures to the heart is closed; but, as the arteries remain pervious, and continue to transmit blood into the extreme veins of the member, the force derived from the heart and arteries will continue to throw blood into these vessels, until it shall be balanced by the resist-

ance opposed by the distension of the coats of the veins beyond the point of compression; and the permanent operation of the same cause will maintain the same state of distension so long as the bandage remains.

But when one of the veins only of the arm is tied, the case is extremely different. Other passages are still open between the blood in the part of the vein behind the ligature and the heart.—The more direct road is indeed intercepted; the suction influence of the heart will not reach it through the usual channel but will still be extended to it through another but a more circuitous route. The blood, contained in the part of the vein beyond the ligature, is intercepted by the compression of the vein from the exhausting influence of the heart; it sustains, on the side of the ligature, the full weight of the atmosphere; but the particles further removed and situate at the entrance of the nearest anastomosing vessel are less resisted by the blood in this vessel than by that between them and the ligature; they therefore give way and take the least resisted course; other particles succeed to their place; and, in turn, yield to the same influence, till at last the whole part of the vein, between the liga-

ture and the nearest anastomosing branch, is completely emptied, and must remain so, as long as the ligature compresses the vein.

The course of the globules of blood, observed to become retrograde in the pellucid parts of animals, admits of a similar, and no less satisfactory, explanation. Should a small vein be for a time obstructed, which may reasonably be supposed often to happen, in consequence of pressure, or even by the magnitude of some of the globules themselves, the globules behind the obstruction will be affected in the same manner that the blood has been observed to be, in the part of a vein behind the ligature by which it had been bound. It may easily be conceived that the blood, in the small vessels, particularly in those which form a communication between the branches of one stem and those of another, will flow in different directions, without the supposition of any obstruction. The supplies of the vessels belonging to one branch may not be sufficient for their demands, while the supplies of another may at the same time be superabundant. The resistance opposed to the entrance of the blood into these different sets of vessels will vary according to circumstances. The blood in the

connecting vessels will uniformly pursue the less resisted course, and will therefore move through these vessels, sometimes in one direction and sometimes in another. Hence we may account for that oscillatory motion which is observed in the capillaries and which was supposed, by that excellent physioligist, Dr. Robert Whytt, to be the cause of the passage of the blood through the small vessels.

It is impossible at this part to withhold our admiration of the simple, yet most effectual, manner in which provision is made against the stagnation of blood in case of any obstruction in the veins, and its various consequent mischiefs. If it had been propelled by causes which must always drive it in the same direction, the blood must have often been accumulated and become stagnant, and in a short time putrescent; especially when it is considered that the veins are very dilatable, and have no power in themselves of reacting against the distending force. But so long as there is open a communication between the blood, in any part of a vein, and the heart, however circuitous that communication may be, the stagnation of blood, either before or behind the obstructed part, is not to be apprehended.

In the arteries the same mischiefs are prevented by the contractile energy of these vessels displacing the accumulated blood, or correcting the error by the more troublesome and often dangerous remedy of exciting inflammation in the part.

It was remarked in the numerous experiments that were made by surrounding the veins of living animals with ligatures, that, in some cases, the swelling behind the ligature was considerable and of long duration; that, at other times, it was slight and soon subsided. These appearences may all be explained from the difference with respect to the proximity, number and capacity of the anastomosing vessels connected with the part of the vein behind the ligature. For if these anastomosing vessels be at a short distance, numerous and large, no tumefaction will take place behind the ligature, or it will soon subside; but if they are remote, rare, and of a minute calibre, the tumefaction will be greater and more durable.

It was observed that the extremities of the arteries and veins were supposed to communicate in general directly, but that it was ac-

knowledged, that in some parts of the fabric, they form communications, through the intervention of cells. These parts are chiefly, the corpora cavernosa penis, the clitoris, the uterus and the mammæ. By what agency the cells of these parts are emptied, and the blood which they contained is returned again into the circulation, after the causes by which they were filled have been withdrawn, is a question that has been much agitated by physiologists. The *vis a tergo*, proposed by Dr. Harvey, is not adapted to the end, and supposes a continuance of the causes by which the cells had been filled. Nor have Dr. Brown Langrish, and the advocates of the venous absorption, been more fortunate in their explanation of the phenomenon, as they have not been able to shew by what means the veins are rendered capable of acting as absorbents. The elasticity of the cells cannot be supposed singly to produce the effect; for, as was justly observed by Dr. George Fordyce, the cells are not tense at the time of their depletion. After examining maturely every hypothesis that has been advanced, this last mentioned author concludes that the phenomenon is inexplicable on the principles of hydrostatics. "For supposing," say he, "an opening made in a vein, there is a pressure equal

to the circulation in the vein, to force the blood out of this opening, it would therefore flow out and remain out, unless there was a force superior to this pressure to throw it in again. But we know of no such force in the veins of the common structure." But the force for which the learned author searched in vain, and the discovery of which he considered unattainable, has, I trust, at last been found. In consequence of the causes by which the cavities of the heart are expanded and of which the explanation need not here be repeated, less resistance is opposed to the blood at the opening into the vein than at any other part of the spherical cavity. The elasticity of the cell and of the substance surrounding it, and even the specific gravity of these parts will co-operate with the abstraction of resistance in the direction of the venous orifice, and effectually produce the evacuation of the cell upon the removal, or adequate diminution of arterial force, by which the cell had been filled and distended.

How certain emotions of the mind should circulate through the arteries terminating in these cells, an increased quantity of blood, or should for a time obstruct the ordinary return of

it from them by the veins, is a subject into which we shall not presume to enter.

Upon the same principles, the rapid contraction of the uterus, after the delivery of the child, may be plainly, and it is hoped, satisfactorily explained.

When the sides of a recent wound are united, and kept in that situation for some time, a communication will be formed between the vessels on one side of the divided portion and those of the other. The mouths of the wounded vessels become united by vessels of the same kind, newly formed for the purpose, and the wound is healed. If a tooth be taken out of the jaw of one person, and instantly inserted into the place from which the tooth of another person has been extracted, the newly planted tooth will take root, and will live in the head into which it has been transplanted. The spur of a young cock may be transferred to his comb, where, in consequence of the superior richness of the new soil, it will flourish with fresh vigour and far outstrip its fellow, which had been left on the native but more barren soil of the other leg.

A piece of the flesh which has been cut out of the body of one animal and instantly applied to the fresh wounded surface of that of another, so that the two divided surfaces remain in contact, will unite with that other; and any injury afterwards done to this newly ingrafted portion, will excite pain as sensibly in the animal to which it now belongs, as it would have done to its first possessor, had it been inflicted before its separation. The practice and recommendation of this art have conferred a ludicrous immortality upon the name of Taliacotius.

These curious phenomena depend upon principles which, though sometimes fanciful in their application, are of the greatest benefit to animal existence. Without them, the list of incurable diseases, already alas! too long, would have been greatly extended, and the loathsome affections of life multiplied beyond calculation. The process, which nature pursues for the union of divided parts, has always excited the eager attention of the curious observer. The late ingenious and indefatigable Mr. John Hunter bestowed particular attention upon this curious and important speculation. This celebrated physiologist, in the course of his labours, was led to the conclusion

that the blood not only possessed that species of life by which it resists the chemical attraction subsisting between its constituent parts, but also, that in certain circumstances it had the power of forming itself by its own action into a vascular structure. Mr. Hunter supposed that when a wound is made and the lips of it brought together, the blood oozing from the mouths of the divided vessels, exerts its creative powers and is converted into new vessels, which form a channel of communication between the vessels which had been separated. The communication between the vessels being thus established, and the plastic nature of the gelatine of the blood uniting the other parts, the circulation is restored and the wound healed.

But it would not, upon this reasoning, be sufficient to allow the mass of blood the wonderful and improbable properties here ascribed to it. It must be supposed to possess others still more singular. It must have judgment to fix the proper direction of the vessels, which it has formed from itself. For it has been often remarked that a vessel, finding upon the surface immediately opposed to it no vessel with which it can form a communication, goes in quest of one; and it is

at a distance frequently that it finds the object of its search. In fact the new vessels which form the communication between the mouths of vessels opening on divided surfaces are observed to pursue an oblique, crooked, or even a circular course. Before the time of Mr. Hunter, it was supposed that the arteries in these cases became elongated, and that it was by the stretching of the old, not by the formation of new, vessels, that the vascular communication between divided surfaces was restored. But this is contrary to observation; the vessel which unites the mouths of two opposite vessels, Mr. Hunter ascertained, from an examination of the process in its various stages, to be a new formation. Besides, the same difficulty occurs upon this supposition respecting the direction of the elongated vessel, and the faculty of being able to judge upon the course that is to be taken, would be as necessary to the elongating vessel, as, upon Mr. Hunter's supposition, it is to the blood itself.

Let us attentively consider the means by which the blood is circulated, and examine whether a more simple and satisfactory explication of these phenomena may not be found.

When a wound is made in the body, and the parts brought together and kept in that state, the plastic nature of the blood that had oozed out of the arteries, drying near the surface, unites the edges of the wound. But the blood oozing out of the arteries below the surface remains liquid. The veins in the mean time have become emptied in consequence of the blood which they contained having taken the less resisted course and returned to the heart. The blood then, which had flowed from the mouths of the arteries into the interstices between the divided surfaces, being less resisted towards the mouths of the veins than in any other direction, necessarily enters the veins and continues its course to the heart. Other blood follows, and thus a communication is established between the artery and the opposite vein. The blood forming this slow current, coagulates upon the external surface, and in due time a vessel is thus formed between an artery on one side and a vein on the other side of the wound.

Thus the formation of new vessels communicating between the arteries on one side and the veins on the other side of a wound, with a direction conformable to the relative position of the

mouths of these vessels, is easily explained upon this simple principle, the tendency of a fluid to restore the equilibrium of pressure between the particles which compose it; and without being under the necessity of ascribing to the blood properties and even faculties, which it is impossible to conceive that it possesses.

But it may be urged that this explanation will not apply to all the cases. In this instance there are on each side arteries and veins, the communication between which and the heart has not been interrupted. But in the instances of the transplanted tooth, of the ingrafted spur, and of the transposed flesh, the communication with the heart on one side is completely cut off.

In reply to this objection, it is contended that in the case of the tooth, the cavities of the blood vessels with which it is supplied, from the incompressible nature of the substance of the tooth, are always of the same dimensions. If, therefore, a diminution of pressure, which was previously equal, be made at one end of a small incompressible tube, and if the other end of the tube be inserted in a liquid, a current will necessarily be produced from one end of the tube

to the other, This canal may be supposed to perform the function of a vein, and its demands, as they are perpetually renewed, will be supplied by an artery on the other side of the tooth; the same will take place in all the vessels of the tooth; so that a communication will be established between the tooth and the parts which surround it; the circulation will be restored and the life of the part preserved. The same reasoning may be transferred to the transplanted spur of the cock.

But it may still be urged that this reasoning is not applicable to a piece of flesh which has been inserted, as the vessels belonging to it are compressible, and their tubular form not so easily preserved. It is to be observed in this case, that though the larger veins collapse upon abstraction from the body, yet the capillary veins, though possessed of the same structure, may, in the same situation, be supposed to preserve their tubularity in a considerable degree, in consequence of the minuteness of the circles formed by their composing fibres and the great curvature and proximity of the tubular arches. As the transverse section of the larger arteries forms a complete circle, the minute arteries may be ad-

mitted to preserve their tubularity in a still more perfect degree. The blood which is poured from the arteries, the connexion of which with the heart has not been interrupted, into the interstices between the approximated and adherent surfaces, will in time accumulate and be compressed by the resistance opposed by the two surfaces.

The transplanting of glandular bodies, as of the testes of animals, may be still more certainly effected, as the vessels of these parts preserve their tubularity more perfect, and continue more permeable, after their separation from the body.

Hence from the conjoined operation of the pressure affecting the extravasated blood, and of the exhausting influence extending to it from the heart through the veins, in connexion with the capacity which the small vessels possess of preserving their tubularity and of the adherence of the principle of life to the newly applied part, the circulation between that part and the adjoining substance will be gradually established, their union effected, and the life of the former preserved.

Thus, from the principles which have been attempted to be established in the preceding pages,

there is derived an easy, and, I hope, satisfactory explanation of the manner in which the parts of a body that have been divided are brought to unite; and a step seems to have been made towards ascertaining the process which nature pursues in the greatest of all her works, the formation of animals.

It has been remarked, that valves are stationed at the passages between the auricles and ventricles, and at those between the ventricles and the roots of the arteries, but that no valves are discoverable at the entrances of large veins into the auricles. The uses of these valves are now considered sufficiently evident. But no satisfactory reason can be drawn from the explanation of their offices given by the illustrious Harvey, or by any subsequent physiologist, why valves should not be as necessary at the roots of the veins, to prevent the regurgitation of blood during the contraction of the auricles, as at the passages in which they are found, to prevent the reflux of blood from the ventricles and arteries during their contraction. Some authors, impressed with the necessity of their existence, have maintained that they have seen a species of valves at the root of the vena cava. But their assertions have not been

confirmed by general observation. And the valve which could serve the purposes which are assigned to it, must upon all occasions have been too evident to escape the most careless examination. But, if the causes, to which the motion of the venous blood has been attributed in the preceding pages, are real, it will be found, that valves, stationed at the roots of the large veins would be altogether useless, and their existence inconsistent with the usual œconomy of nature. Synchronous with the contraction of the auricle is the dilatation of the ventricle. Consider the causes by which this last chamber is dilated.—They are not the forces, which are supposed to proceed from the contracting auricle, and which singly would, unquestionably, drive the blood as forcibly back into the cava as forwards into the ventricle; they are derived, in part, from the simple relaxation of the fibres, which is necessarily accompanied with an expansile effort; but chiefly from the elevation of a share of that pressure, to which all the objects on the face of this globe are subjected, from the convex surface of the heart. The blood, therefore, pressed upon by the contracting auricle, necessarily takes the less resisted direction, and is all drawn into the ventricle. The difference of resistance which

the blood has to encounter in the two passages serves all the purposes of the obstructing valve, which, under different circumstances, would be required at the root of the vena cava. But admitting even that the valve, after which anatomists have so long searched with scrutinizing eyes, were to be found, it would not serve the purposes which they have had in prospect. Supposing a valve to be stationed at the opening between the cava and the right auricle, and so constructed as to oppose, in similar circumstances, the passage of a fluid from the auricle into the vein; and suppose that the auricle, which has just been filled from the vein, contracts, what in this case is to shut the valve? There is no diminution of resistance or suction influence, which is necessary for the favourable play of valves, on the side facing the vein; on the contrary, the blood is supposed to advance against it with considerable momentum; and the pressure which is made upon the valve by the contracting auricle, is equal on both surfaces. The valve therefore would as likely be continued open as closed.

A slight degree of reflexion will be sufficient to convince the intelligent observer how much

more important the offices of the valves which are found in the heart will become, and how much more certainly the regularity and efficacy of their action will appear to be secured, upon the supposition that the chambers of the heart are dilated by the causes which, in this treatise, have been assigned, to that purpose.

The two trunks of the ascending and descending cava meet at the heart in such a manner as to form a straight line, which caused them at first to be considered as one vessel and to be designated by the common appellation of vena cava. The streams of blood, which are conveyed by these vessels to the heart, are placed at that point in direct opposition. Upon the supposition that the blood is returned to the heart by a *vis a tergo*, by the action of the coats of the veins, or of those of the concomitant arteries, or by the compression of the muscles, this position of these vessels is the most unfavourable that can be conceived, for the office that is assigned to them. The momentum of the blood in one vessel would be destroyed by that of the other; or, if the current in the descending was stronger than that in the ascending cava, the blood in the weaker stream would be prevented from ever reaching

the heart. The difficulties, opposed to the return of the blood from the extreme vessels to the heart, already upon these hypotheses, insurmountable, would be accumulated beyond calculation, by this location of the venous trunks.

All these objections vanish from the view that has been presented of the causes of the venous circulation; and, in their stead, advantages become strikingly displayed. As a share of the ordinary pressure is removed from the blood in the roots of the vena cava by the reiterated actions of the heart, this organ becomes the centre to which all the blood in these vessels is pressed; and the two apparently opposing streams, at the moment in which they are about to make a collision, are each directed, by the exhaustion of the heart, without sustaining any resistance from the other, into a common reservoir.

In certain circumstances, great advantages must result from this position of the venous trunks. As the impetuosity of the currents, by the conflux of which the streams of the cava are composed, is liable to be greatly augmented by the actions of the superincumbent muscles during violent exertion, a greater quantity of blood may

be returned than the heart is fitted to receive, and the organ would be in danger of sustaining violence from the augmented impetuosity of the current. In such a circumstance, the momentum of the blood in one trunk will be expended against that of the blood in the other; and the heart in a great degree defended from violence.

As the blood in the system of the pulmonary veins is not exposed to such transient accelerations, the left side of the heart does not require to be protected from violence in the same manner.

The heart impinges against the left side a stroke, which is not only very perceptible to the touch, but at times excites a sound which is sufficiently audible. The cause of the beating of the heart against the chest has frequently attracted the attention of the curious, and has been the source of considerable controversy.—The opinion, which most naturally arose upon a slight consideration of this phenomenon, was, that the stroke was produced by the blood propelled forcibly against the chest by the contraction of the left ventricle. An examination of the course of the aorta, which does not approach with any of its divisions, the part of the chest

generally impinged, presented an undeniable refutation of this hypothesis. It was next urged that the walls of the ventricles were pressed forcibly, during their expansion, against the sides, by the impulses of the contracting auricles; and that thus the stroke in question was produced.—The observation, however, that the stroke was not made during the dilatation, but during the contraction, of the ventricles, became fatal to this supposition. An explanation was attempted to be derived from the momentum of the blood in the veins; but it was not easy to describe how a power acting constantly and uniformly, like that of the force of the venous blood, could produce an intermitting impulse. The late renowned Dr. William Hunter proposed, in his lectures on anatomy, a new explanation of this phenomenon; which is certainly ingenious, which received the ready assent of the admirers of that great man, and which is at present generally acknowledged to be satisfactory. This explanation, as it is given in a note contained in Mr. John Hunter's treatise on the blood, which was edited by Sir Everard Home, is as follows: "By its (the heart's) throwing the blood into a curved tube, viz. the aorta, that artery at its curve endeavours to throw itself into a streight line to increase its

capacity, but the aorta being the fixed point against the back, and the heart in some degree loose or pendulous, the influence of its own action is thrown upon itself, and it is tilted forwards against the inside of the chest." But this explanation, ingenious and plausible as it may appear, will not, it is contended, be found satisfactory upon minute examination. The position of the heart it is admitted could not be affected by the projection of blood singly out of its cavities. It is not supposed in this explanation that the heart was affected by the reaction of the blood, as in that case the blood must be considered to have moved at the time in a retrograde course, and for that purpose the straight would have been equally favourable with the curved line of the artery. With respect to the effects ascribed to the curvature of the aorta, it may be observed that no proof appears to be adduced of the supposed change taking place at the time; that, from an examination of the connexions of the aorta with the parts with which it comes in contact in its course, it is not probable that such a change could be easily produced; and that, allowing the vessel to be quite free and unattached, it would not be brought into a straight line by the impulse of the blood flowing through

it; as it is the law of fluids moving in tubes to press equally on all sides. A satisfactory illustration of this law is daily exhibited in the conducting of water from one place to another in pliant flexible tubes. These pipes, though they may easily be extended into a straight line, and though a fluid may be impelled through them with great impetuosity, retain the position, however curved it may be, in which they have been accidently placed. But admitting the force of the blood to have a tendency to restore the curved aorta into a straight line, it is not conceivable that such a degree of rigidity could, by that means, have been given to that vessel, as to enable it not only to elevate, by an unfavourable lever, so massy a substance as the heart, but to drive it forcibly against the chest.

Let us inquire, whether a more easy and less exceptionable explanation of this remarkable and interesting phenomenon, may not be deduced from the principles which we have in these pages been endeavouring to establish.

It has been stated that the stroke which the heart impinges against the side is synchronous with the systole of the ventricles. The apex of

the heart is brought nearer the base, and the dimensions of the body of the heart are decreased by the contraction of the ventricles. Within a space of time too short to admit of measurement, the auricles are augmented in an inverse proportion. A small portion of the pericardium covering the apex of the heart is, in the ordinary position of the body, in contact with the side. During the contraction of the ventricles, the apex of the heart is drawn vividly and powerfully from the part of the side to which it is opposed, and from which it is separated only by the pericardium; and, in consequence, the portion of the side is drawn inwards, so that a slight depression is occasioned in the intercostal muscles; but, on account of the greater facility with which the blood in the veins can be made to change its place, and of the impulse which it sustains either from the residue of the *vis a tergo*, from gravity, or from the contracting roots of the cava, the auricles are dilated, and the apex of the heart is not only rapidly restored to the place from which it had, in a slight degree, withdrawn itself, and which had been occupied by the yielding substance of the side; but an impulse from within is now to be sustained by the parts which, the instant before, had been drawn in an opposite direction. The

range of movement which the yielding parts of the chest have to traverse in an outward direction, is lengthened, and the stroke which, in consequence of that movement, will be impinged against a body placed upon the external surface of these parts, must be proportionably more perceptible.

Some circumstances, connected with the beating of the heart against the chest, require a more particular notice.

The stroke of the heart is not always felt at the same part, and, in some situations of the body, is not felt at any part of the side.

In consequence of the weight of the heart, of its being in a great degree loose and pendulous, and of the range which the pericardium will give it, in the various directions in which it may be urged by gravity; the heart will slightly vary its place and position, with the varying positions of the body. When we place ourselves upon the right side, the apex of the heart will recede to a certain extent from the part of the side to which it points, and the left lobe of the lungs will be extended downwards, between it and the side.—

In that case, the impulse to be sustained by the movement of the heart, will not, in ordinary circumstances, extend to the side, and other positions of the body will give the apex of the heart a direction more elevated or more depressed than the common.

Nor will the place of the chest against which the stroke is made be varied by changes in the position of the body alone; it will be altered by variations in the force and in the balance of the circulation. The impulse, which the body of the heart sustains and which is supposed to impel it against the side, proceeds from different points. The direction of the diagonal of the parallelogram formed by the currents from the cava and pulmonary veins, will be varied by the direction and force of those currents; but the body of the heart will be impelled against the chest in the direction of this diagonal.

When we consider the vehemence of the stroke which the heart impinges against the chest, and at the same time reflect upon the injury which the more solid parts, and even bones, in a short period sustain from the beating of an aneurismal artery, we are struck with surprise that the left

side and the heart itself do not exhibit marks of extensive destruction, from the long continuance and frequent repetition of the collisions that take place between them.

The mode in which the heart and the parts impinged by it are protected, during these movements, from that injury which, from analogy of effects, was to have been inferred, may be collected from the preceding statement. Before the yielding part of the side has been pressed outwards, it had been drawn to a certain extent from its natural situation in the direction of the heart. The whole range of its outward movement is composed of a resilient and an impelled action.—No injurious impression can be sustained by the side during the share of the movement by which it recovers its natural position; and it is only after it has passed the line of that position, that any violent impaction can take place between it and the heart. The positive stroke, made against the side by the heart, is not so great in reality as in appearance, and as the extent of the impulse sustained by the hand would indicate.

Another abundant source of protection, during these movements, is derived from the frequent

change of the part of the heart by which the impaction is made, and of the part of the side which sustains it. The bone, against which an aneurismal artery beats, is never for a moment relieved from the pressure of its action. According to the hypothesis of Dr. Hunter, the part of the back which served as a buttress to the aorta would always, and the part of the side against which the heart was tilted would generally, continue the same. But, according to the explanation of the phenomenon which we have ventured to propose, both the instrument by which the blow is inflicted, and the objects sustaining it are changed by every variation in the position of the body, and in the relative forces of the greater and lesser circulation.

It will readily be acknowledged by those whose occupations, or accident, have kept the body long in the same position, that, on such occasions, they have frequently felt, in the left side and in the region of the heart, an uneasiness which was soon removed by exercise, or change of position.

There is, in the circulation of the blood through the head, a peculiarity which it may be proper briefly to notice. It is evident that the dimen-

sions of the cranium are fixed. As, further, the substance of the brain itself and of the membranes is unchangeable, and as the pressure among the contents of the cranium must be the same with that to which the other parts of the body generally are subjected, the head will contain at all times nearly the same quantity of blood. The veins which take up the blood from the membranes and substance of the brain, empty themselves into vessels of a very peculiar structure. These are certain membraneous reservoirs called sinuses of a triangular form, and of an invariable capacity. The sinuses of the brain communicate with the internal jugulars and other veins which convey the blood to the superior cava; and whence it is transmitted to the heart. Suppose, as must at all times necessarily be the case, that all the vessels within the head are filled to that degree, which would bring the contents of the cranium generally to an equilibrium of pressure with the other parts of the body, and suppose a fresh quantity of blood to be, in these circumstances, thrown into the head by the carotids; in order that the equilibrium of pressure may be still preserved, a quantity of blood equal to that impelled into it by the arteries must be displaced from the head by a synchronous

movement. The return into the arteries is impracticable; it must be discharged from the sinuses, the only other outlets which are afforded, into the veins of the neck. These sinuses, as their sides cannot be compressed without great force, must have imbibed, at the same time, a quantity equal to that which they had discharged, from the small veins which enter them from the membranes of the brain.

An impulse is thus transmitted, by means of the action of the arteries, which supply the contents of the cranium, to the blood contained in the veins, which return it from those contents, similar in effects to the *vis a tergo*, but after a manner that is peculiar to the circulation of the head. The motion generated in the blood contained in the sinuses, by the tendency in the contents of the cranium to preserve the equilibrium of pressure, accordingly as the projectile force of the carotids tends to derange it, keeps the veins of the neck in a proper state of distension, and places the blood, as it may be demanded, within the sphere of the exhausting action of the heart.

By circulating the blood through the head in this manner, protection is admirably afforded to

the slender vessels of the brain from being ruptured by the force of the circulation; and the substance of the brain is maintained in that uniform state of compression, and in that equality of dimension, which appear necessary to the proper discharge of its functions.

As the lungs of the unborn fœtus are collapsed and at no time in a state of forced dilatation, and as the circulation is known to be performed as well before as it is after the birth of the infant, it may be urged, that the chambers of the heart are dilated in a situation in which the elasticity of the lungs can have no influence, in removing a share of atmospheric pressure from the convex surface of the heart or pericardium; that, therefore, the blood is returned to the heart of the fœtus without the assistance of those causes which have been considered necessary to the accomplishment of that end; and that, as the circulation can be perfectly performed in one condition, without the interposition of those causes, it is unnecessary and unreasonable to have recourse to them in any other. This objection, plausible as it may appear at first view, may easily be proved to be without any weight. But the attempt to remove it, as it necessarily

involves the consideration of the actions of the diaphragm, is properly deferred till after these actions shall have been examined, according to our purpose, in the appendix.

As the weight of the atmosphere has evidently an important influence upon the motion of the blood, it is reasonable to expect, that this influence will be modified by the amount of that weight; and that, in general, the living body must be materially affected by any great diversity in the weight to which it is subjected by the atmosphere. The slighter changes of atmospheric pressure, arising from changes in the general amount of that pressure, and usually indicated by the barometer, though capable of influencing the movements of the frame, in a degree which renders itself sufficiently perceptible to the feelings, and which is manifested by the flow of spirits and the bodily agility, are not calculated to produce effects suffciently palpable for our examination. For that purpose, we must have recourse to situations where the difference between the pressure and that to which our bodies have been accustomed is more extensive and permanent. Such a diminution of the weight of the atmosphere, as is fitted to produce effects suffi-

ciently observable to attract the notice of the philosophic traveller, is to be found on the summits of the most elevated mountains. The effects, which are produced upon the body by great elevation into the regions of the atmosphere, were experienced by Saussure and his companions in their journey to the top of Mount Blanc, and have been described with elegance and perspicuity by that celebrated traveller.

It appears that, when Saussure and his fellow travellers had advanced to a considerable height in the mountain, they were seized with excessive fatigue which obliged them frequently to rest. Many of them were seized with faintings from which they completely recovered after lying upon the ground a short time. The pulse was quick and small; the tongue parched; the thirst great; the respiration laborious. Though the fatigue was excessive, and obliged them instantly to stop and rest, it in many respects differed from that fatigue which is experienced after labour in lower regions. It was suddenly induced to the highest pitch, and as suddenly removed. After they had rested a few minutes they arose as fresh and as alert as they had been at the commencement of their journey. But before they had

proceeded many yards they were obliged to rest again. It was remarked that this difficulty in ascending happened at a lower part of the mountain to some people than to others. Saussure does not describe the persons who were first affected in this manner, but says, that some men, who appeared very strong and capable of great labour without fatigue, could easily reach a certain height, but could ascend no further; that, when they attempted to go beyond this limit, they were seized with faintings. Some could advance higher than others without suffering any inconvenience. He remarks further, that they were frequently seized with slight hæmorrhages from the lungs; that the whole body was unusually turgid and red; and that the blood vessels were uncommonly full.

From an account of a journey to the Peak of Teneriffe, contained in the European Magazine, for December, 1813, the following is an extract:

" In ascending" says the traveller, " the highest part of the mountain, called the Sugar Loaf, which is very steep, our hearts panted and beat vehemently so that, as I observed before, we

were obliged to stop above thirty times to take rest. But whether it was owing to the thinness of the air causing a difficulty of respiration, or to the uncommon fatigue which we suffered in climbing the hill, I cannot determine, but believe that it was partly owing to one, and partly owing to the other. Our guide, a slim agile old man, was not affected in the same manner with us; but climbed up with ease, like a goat; for he was one of those poor men who earn their living by gathering brimstone in the cauldron and other volcanoes."

M. Saussure has attempted to give an explanation of these singular and interesting phenomena; and is of opinion that they arise from the rarification of the atmosphere in these high regions. He argues, that, on account of the lightness of the atmosphere, the volume of the air is greatly augmented; that therefore a given bulk of this fluid contains less oxygen at the summit, than at any lower stage in the mountain; that a sufficient quantity of this substance, which is well known to be necessary for the continuance of exertion and of life, is not in a given time taken into the system by the action of respiration, for the purpose of the body under exercise;

and that the fatigue arises from the want of the due oxygenation of the blood.

But many strong objections may be urged against this explanation. It is to be observed that the expansile effects of levity upon the air in these high regions are in a great degree counteracted by the excessive cold, by which the elasticity of the fluid is diminished; that, in consequence, the difference of oxygen, contained in equal bulks of air at the top and bottom of a high mountain, is not so great as was to have been expected by attending alone to the weight of the atmosphere in these different situations; that the symptons which have been described are not usually exhibited by persons breathing an air heated to such a degree as to render it rarer than it exists at the top of the highest mountains, and are not such as are usually produced by breathing air containing a deficiency of oxygen; which occasions stupors and other apoplectic symptoms; that a greater quantity of oxygen is required by the system during labour than rest, is an hypothetical opinion; that the great want of oxygen here supposed should be suddenly removed from the system by the deficiency of expenditure, arising from a state of rest alone,

without any increase in the supply of the substance, is altogether unreasonable; and that the slight hæmorrhages from the lungs, the redness of the skin, and the swellings of the blood vessels, cannot be explained from a deficiency of oxygen in the system.

It will be found, I trust, that the inconveniences which travellers experience, in ascending to the summits of high mountains, are to be traced rather to mechanical than to chemical causes, and that they arise not from the rarity, but directly from the levity of the atmosphere.

It is well known that the pressure of the atmosphere is less upon the summits of very high mountains than on the plains that are nearly level with the sea. If the weight of the atmosphere has upon the motion of the blood really that influence which has been ascribed to it in the preceding pages, it was to be expected that the motion of this fluid, and of course the actions of the animal machine generally, must be materially affected by any considerable difference in the amount of that weight. The conclusions, which are fairly deducible by reason alone, are, in this instance, fully confirmed by observation.

The singular affections, experienced by Saussure and other travellers, who have ascended into very elevated regions, are supposed to have been produced in the following manner.

As the weight of the atmosphere is supposed to have an important share in producing the diastole of the heart, it is reasonable to infer that the motions of this organ will be affected by any considerable alteration in the amount of this weight. To produce the effect required, it is necessary that the pressure should be sufficient to balance, not only a column of blood of several feet in height, in circumstances in which the resistance arising from gravity is augmented by the tenacity of the fluid and other causes; but also to overcome the inertness of the strong substantial muscle inclosing the chambers of the heart. It may be admitted, that on the summits of the highest mountains which are placed on the face of this globe, there would still be found a sufficient residue of pressure for accomplishing these purposes; but it may be conceived to be so far diminished, as to become incapable of restoring the diastole of the heart with the ordinary rapidity, and with that energy which is consistent with health. In such circumstances the func-

tions of the heart would be more feebly discharged, and the circulation become more languid.

It was remarked in a former part of this treatise, when the effects of muscular action upon the motion of the venous blood were examined, that the circulation in the veins would be retarded by the pressure of the muscles upon the subjacent veins, in consequence of the aggregate of the bores of the vessels being greatest on the side of that pressure most remote from the heart. If further a considerable diminution in the energy of those powers, by which the blood is returned to the heart, be accompanied by the obstructions which violent exercise is calculated to oppose to the circulation at this part, the blood will not reach the heart in sufficient quantity, but will be accumulated in the extreme vessels. Hence evidently would proceed faintings, suddenly overwhelming fatigue, redness of the skin, great distension of the superficial vessels, and slight hæmorrhages from the lungs.

The laborious breathing, ranked in the train of affections which the travellers suffered, may be traced to similar causes; but will more evidently appear to be a necessary accompaniment of the

ascent into very high regions, after the causes of the actions of the diaphragm shall have been, according to our intention, considered in the appendix.

A very remarkable characteristic of the lassitude with which the travellers to the top of Mount Blanc were affected, in so overwhelming a degree, was, that it was as suddenly and as completely removed by rest. After resting a little, they found themselves as alert and vigorous as they had been at the commencement of the ascent.

Two causes seem, at that time, to have conspired in restoring the activity of the body. The first was the horizontal position which the extreme lassitude and faintings forced them, more or less, to assume at short intervals. In this position the column of fluid, required to be balanced by the atmosphere, was greatly reduced, and the blood, of course, more vigorously transmitted to the heart. Its determination to the centre of the system would also be favoured, in a greater degree, by gravity. The other cause was the cessation of muscular action; as then the *vis a*

tergo ceased to be counteracted in the veins by the pressure of the muscles.

As the lassitude was not induced by muscular action repeated with vehemence and for a long time; which would have exhausted the irritability of the muscles, and required a considerable period of repose for the recovery of that irritability; but by a sudden abstraction of the ordinary stimuli, the moment at which the stimuli were re-applied in the usual manner, the excitement of the system was renewed, and the vigour aud activity restored.

It is worthy of remark that the lassitude came upon some of the travellers sooner than upon others; and that, in some instances, there was a defined state of elevation above which they could not ascend. Every attempt to conquer the higher ground proved abortive. The regions above a certain point seem to have been unfit for their existence. Saussure has not particularly described the persons who were first affected with unconquerable lassitude; but observes, that men, who were very robust, and who in other circumstances could undergo great labour without inconvenience, were soon subdued by this species

of exertion. The guide who conducted the travellers to the Peak of Teneriffe, a little slim old man, was not affected in the least. All other circumstances being equal, it is presumed that the tallest and most robust would soonest reach the limits of the region fitted for their existence, as in them the column of blood to be supported and moved is higher and more resisted.

It follows, from what has been stated, that this earth is not habitable by animals above a certain magnitude; and that, as the weight of the column of fluid contained by the animal machine approaches nearer to a balance with the weight of the atmosphere, the circulation through that machine must be more languid. Little animals would appear to be best adapted to the atmosphere of high mountains. Accordingly we find that animals which live upon very elevated mountains are less than those of the same species which dwell upon the plains. Other causes may cooperate in producing this differeuce. Birds which ascend into the higher regions of the air should be less than the animals which reside continually on the surface of the earth. Fishes, again, which in addition to the pressure of the atmosphere support that of the water in which they

swim, and which approaches in its specific gravity to that of the blood, may exist almost of any size, provided that there is sufficient depth in the water.

The benefit of sea bathing may, perhaps in a certain measure, be ascribed to the additional energy that is given, for the time, to the circulation of the blood, by the weight of the water.

The operation of the causes, to which the blood has in these pages been ascribed, is admirably calculated to adapt with precision the demands of the circulation at all parts to its expenditure; and particularly to provide a sufficient supply of blood to the heart against the time that it may be required. Had the heart affected the blood by its projectile power alone, that fluid must, in its progress through the arterial system, have in time become independent of the power by which its motion was originated; and a thousand causes might intervene, over which the heart, so limited in its influence, could have no controul, to retard or divert its course; and which by occasioning one short fatal delay might prevent its return for ever. But in reality the heart extends its empire over the whole circulating system; it maintains

a regulating influence over the blood, through the whole of its course, and directs its return to itself in the veins with the same efficacy that had insured its departure in the arteries.

In the mode in which the blood is circulated, we have to admire the provision which is made against a deficiency being sustained, under almost any circumstances, at the heart. For this organ is made the centre of pressure and of gravity, and designates the stage in the circulation in which a deficiency of supply would be the last in being felt. Hence it happens, that the functions of the heart are performed, and life preserved, notwithstanding long and copious discharges of blood; which, upon every other hypothesis, must have proved fatal. For according to these hypotheses, the heart, or at least the auricles, are placed at the end of projection, they mark the highest advance of the tide, and would first be abandoned by the retiring fluid. They would be drained by every profuse hæmorrhage; and the heart would expend its energy in fruitless efforts, to circulate a fluid that came not within its reach.

Although ample provision is made for a sufficient quantity of blood in due time to the heart,

this organ is by a beautiful contrivance protected from all harm, and is relieved from the injurious impulse which otherwise it must have sustained, in resisting this impetuous stream. We have seen that it is principally by the elevation of a share of the atmospheric pressure from the convex surface of the pericardium, and of consequence from the blood in the venous trunks, that the momentum of the venous circulation is generated; that it is only during their dilatation that this momentum could affect the chambers of the heart; that during this period, yielding to a superior force, the walls of the heart recede and become dilated in all directions; but that, as soon as they have reached that state of dilatation at which their fibres might be overstretched and injured by the momentum of the blood, the pericardium has become dilated to its utmost capacity, and the pressure is equalized, on both the concave and convex surfaces of the heart.—The power which is required to resist the impulse of the blood is at this moment transferred from the heart to the strong tendinous pericardium; and the walls of the heart, at the only state in which they could be injured by the impetus of blood, sustain little more force than the hand does from the pressure of the air with which it is surrounded.

It is by a contrivance no less beautiful and efficacious, that the coats of the veins are relieved from that lateral pressure which, in ordinary circumstances, they must have, sustained, by supporting a column of blood often of several feet in height; and which must have stretched them to such a degree as would have been incompatible with the easy movements of the body, and with the safety of the vessels themselves.—The coats of the veins evidently sustain little lateral pressure. For the purpose of perceiving the cause of this, it is necessary to recollect, that the forces by which the blood is returned from the extreme vessels to the heart are of two kinds; the one centrifugal, derived from the heart and arteries; and the other centripetal, proceeding from the diminution of resistance, in consequence of a removal of a share of atmospheric pressure, in the direction of the heart. By the former of these forces, the coats of the veins are dilated; by the latter, they are pressed inwards; and it is by the proper balancing of these two forces that the veins are placed in that state of easy distension, which they are observed to possess.

To the numerous and important uses which animal existence derives from the atmosphere, an

invaluable addition seems to have been found.—The practical assurance that the air which we breathe is so necessary to us, that life cannot be continued for a few minutes, or even seconds, without it, is coeval with the birth of each individual. Chemistry, that noble science which, even in our days, has been seen to have advanced from infancy to a youthful vigour, and which is daily unfolding new wonders to its delighted and astonished votaries, pretends to explain the nature of that beneficial and necessary change which is produced upon our systems, by respiration; and has been so far successful as to prove that a quality is added to, or impurity abstracted from, the blood in its progress through the lungs; and that this fluid is, by that means, fitted for the performance of its life-preserving offices during another circuit. It is from the air also that the animal derives that surprising and invaluable property of preserving its temperature the same at all times, and independently of the temperature of the atmosphere in which it lives. Though it may not be deemed so necessary to our existence, this service contributes essentially to our comfort, to our health, and to our duration in a climate which is constantly varying; and enables us to traverse, without injury, the burning de-

serts of the torrid zone and the frozen regions that approach the poles.

Scarcely less important is the share which the atmosphere by its mechanical operation contributes to the scheme of life. It is by its weight, that the air, in the process of respiration, is brought into such contiguity with the blood flowing in the fine superficial vessels of the lungs, that the chemical affinities subsisting between their particles are enabled to prevail. We have seen also, that it is to the pressure of the atmosphere that the circulation of the blood is indebted for an important share of the process. To that atmosphere therefore which surrounds us, unseen and unheeded, we are indebted for life in a multiplicity of ways.

In the explication of the phenomena, suggested by the views which have been taken of the causes of the motion of the blood, diseased appearances have been purposely avoided. For it was supposed that an attempt to deduce the symptoms of disease from principles, which, upon the ultimate trial, might be found erroneous, might lay the foundation of a false practice; and produce consequences less pardonable than mere speculative error. It was considered also that dis-

credit might be brought upon principles, in themselves just, by a hasty and perhaps mistaken application of them to the complicated phenomena of disease.

It would be unreasonable indeed to deny that in the progres of this inquiry, novel plans of treating diseases, which are too well known to baffle the skill of the most sagacious and experienced, have occasionally been suggested to the mind of the writer, and have inspired it with hopes, transient indeed, and in all probability visionary, but, for the moment, of the most gratifying description.

It has been a matter of surprise and of reproach against the moderns, that greater benefit has not been derived to the practice of medicine from a knowledge of the circulation of the blood, than has actually arisen from that important discovery. The determined supporter of the superiority of the ancients has still too much reason triumphantly to inquire—In what instance has the practice of physic been improved since the days of Celsus or even of Hippocrates?

Perhaps the disappointment of the expectations which mankind might reasonably have enter-

tained from the discovery of the circulation, may in some degree be attributed to the erroneous opinions which have prevailed respecting the causes of it, and from which copious fountain a poisonous influence has been diffused over the whole system. In proof of this may be adduced the succession of hypotheses to which inflammation has given origin; which have received a currency of greater or less extent and duration, in proportion to the celebrity of their authors; and which could scarcely, in any instance, have existed for a moment with more correct views of the motion of the venous blood.

Another baneful effect which may be attributed to the same cause was the doctrine that the motion of the blood was not governed by the laws of hydrostatics. This doctrine necessarily plunged the whole system of physic into an abyss of mystery. It was ominous of a return of the era in which diseases were cured by spells and incantations.

If the attempt, which has been made to investigate the causes of the motion of the blood, shall prove to have been in any degree successful, a more perfect and distinct knowledge must be derived from it, respecting the nature of those diseases in particular which arise from an unequal

distribution of the fluids; from their unhealthy accumulation in one part, while perhaps they are deficient in another. Nor is it unreasonable to hope, that, if the cause of a derangement be more clearly ascertained, a more efficacious remedy may in time be applied.

Surgery, that branch of the healing art, which has made the most perceptible, and, perhaps because the objects of it are more open to the examination of the senses, the least controvertible progress in modern times, has for its foundation, in an eminent degree, the circulation of the blood. But the modern improvements in surgery, great as they unquestionably are, have, in all probability, chiefly proceeded from a more accurate knowledge of the form and relative situation of the constituent parts of the human frame, both in health and in disease; a knowledge which the prejudices of the ancients opposed, and from the greater perfection attained by the mechanical arts; and have seldom been suggested by any extensive views of the general nature of the animal machine. An advancement however, in our knowledge of any thing connected with the circulation, could scarcely be made without being soon followed by some improvement in the surgical department of the healing art.

Appendix.

THE following pages contain a brief explanation of the mechanism of respiration and of the connexion which is supposed to subsist between it and the circulation of the blood.

Respiration is that function of animals by which a quantity of air is alternately admitted into, and expelled from, the lungs. It is synonimous with breathing. The process by which air is admitted is termed inspiration, and that by which it is expelled, expiration.

The mechanism of inspiration consists in the enlargement of the thorax, which is increased both in diameter and in axis. The axis of the chest is increased, or its cavity is deepened by the movement of the diaphragm. This tendinous and muscular flooring divides the trunk of the body transversely, and separates the chest from the abdomen; it assumes at all times in health a form more or less conical, having its

apex or convex side turned towards the cavity of the chest, and its base or concavity facing the abdomen. It is by the decrease of the convexity of the diaphragm, or by its approach to a plane, that the axis of the chest is augmented or its cavity deepened. The contraction or shortening of the muscular fibres produces the change just mentioned in the form of the diaphragm; they are brought by that means nearer to the direction of straight lines; the altitude of the cone is lessened; and, as the area of its base necessarily remains the same, its apex is rendered more obtuse, or its convexity is decreased. The dimensions of the cone formed by the diaphragm are diminished, and the capacity of the chest is extended in an inverse proportion.

The chest is increased in diameter by the contraction of the intercostal muscles. In consequence of this contraction, the ribs are drawn nearer to each other, and, as the position of the superior row of ribs is fixed, the inferior rows are all drawn upwards. From the peculiar nature of their articulation with the back bone, the ascent of the ribs is necessarily accompanied with an extension of the arches in an outward direction, and, in consequence, with a general increase of the diameters of the chest. But it

is chiefly by the diminution of the cone, formed by the diaphragm that the dimensions of the chest are augmented in respiration.

During the process of expiration the muscular fibres are relaxed. The diaphragm assumes its former extension and shape; the ribs return to the position from which they had been drawn, and the capacity of the chest is reduced to its former dimensions.

As during these changes a free communication at all times subsists between the cavity of the chest and the external air through the windpipe; and, as the other inlets into this cavity are all subjected to regulations which prohibit the admission of a greater quantity of matter into it at one time than at another, to preserve the equilibrium of pressure between the air within and that without the chest, a quantity of this fluid must be alternately expelled and admitted.

The diminution of the altitude of the cone formed by the diaphragm, usually termed the descent of this partition, is universally acknowledged to follow of necessity the contraction of its muscular fibres. The causes by which the altitude of the cone is again restored are not so evident.

In our attempt to investigate the causes of so important a movement, it will be proper to examine previously the efficacy of those powers to which it has generally been ascribed.

The simple relaxation of the fibres of so extensive and thin a substance could unquestionably have no share in the production of this effect. But, for restoring the altitude of the diaphragm, the relaxation of the muscular fibres is necessary to the favourable operation of other powers.

The agents by which the diaphragm is supposed to be elevated (to use the language commonly employed to describe that movement) in respiration, are the abdominal muscles; and the manner in which they are believed to produce the effect is generally explained as follows.

When the diaphragm is contracted, as is acknowledged to be the case in inspiration, the oblique and transverse muscles of the abdomen, are supposed to be in a state of relaxation; and, by permitting the abdominal viscera to protrude, to allow an easy descent to the diaphragm.—When, however, the diaphragm becomes relaxed, either with or without the consent of the will;

the abdominal muscles either in consequence of sympathy, association, or of the stimulus of volition, contract in their turn, and by pressing the abdominal viscera against the lower or concave surface of the diaphragm, cause it to assume a higher elevation in the cavity of the chest.

The abdominal muscles and diaphragm are considered in this explanation to act as antagonists, and it is by the alternations in the contraction and relaxation of these muscles and the diaphragm, that the function of respiration is supposed to be performed.

That the actions of the abdominal muscles should perpetually and invariably alternate with those of the diaphragm, through sympathy, habit, or association, and generally without the intervention of the will, is a doctrine which appears so incomprehensible, that it will not slightly obtain the assent of the inquisitive mind. Great support has been supposed to be derived to the hypothesis of such a connexion subsisting between muscles in every respect distinct, from the alternation in the movements of the ventricles and auricles of the heart. But the aid, which this doctrine is supposed to receive from the analogy

of the heart, is in this case fallacious; for, by referring to the account given in the preceding Inquiry of the actions of this organ, it will be found that the alternations to which allusion has been made, are not produced through sympathy, habit, association, concatenation, or any of that tribe of causes, but that they are the result of a mechanical necessity.

But this doctrine, so improbable in its own nature, receives little or no support from observation. The muscles of the abdomen are not generally during expiration in that tense state, which their contraction to the extent supposed would render necessary. Nor do the abdominal viscera appear to sustain greater pressure during expiration, than at any other period.

In cases of great inanition and collapse of the bowels, the contraction of the abdominal muscles, by bringing those muscles nearer to the direction of straight lines, passing between their attachments—the brim of the pelvis and the lower ribs, would not push the viscera of the abdomen against the diaphragm; but, by enlarging the cavity of the belly to a capacity beyond that which these viscera required, would draw them from the

lower surface of the diaphragm. When the bowels are found protruded in consequence of extensive wounds in the belly, respiration proceeds for some time without more interruption than may be supposed to be derived from pain. But the diaphragm in these cases could not be elevated by the pressure of the abdominal muscles, which, during their contraction, would more easily displace the bowels to a greater extent than elevate the diaphragm. In cases of impaling and in the numerous experiments which have been made upon the lower animals, it is found that respiration may be performed for a considerable time, after the abdominal muscles have been divided, or altogether removed. After death even, when, in dissection, the abdominal muscles have been divided, and the viscera have been taken from the cavity of the belly, the diaphragm is observed to preserve a great convexity, attended with a drum-like tenseness, towards the chest.

If we consider how improbable it is, that a perpetual and invariable alternation in the actions of the abdominal muscles and diaphragm should proceed from the supposed causes; that a doctrine, so incredible in itself, receives little support from observation; and that the effect, which is

supposed in all cases to arise from this cause alone, is certainly produced in circumstances in which that cause cannot operate; we may conclude with the utmost safety, that the contraction of the abdominal muscles is not necessary to the elevation of the diaphragm, and that, in general, it has no share in that effect.

It was ascertained, by an examination of the fabric of the lungs; by the changes which take place in the form and dimensions of those organs upon an opening being made into the thorax; and more particularly by the experiments which are detailed in the Second Part of the preceding Inquiry, that the lungs of a living and sound animal, and after death, so long as its structure remains perfect, are, from the period at which the first inspiration has been drawn, always in a state of forced dilatation; and that, in these circumstances, they are always exerting an effort to resiliate or collapse into the comparatively narrow dimensions which are natural to them. The causes, which prevent them from resuming their natural dimensions, have been detailed in the preceding Inquiry, and need only be slightly noticed in this place.

The lungs, as has been shewn, are dilated into contact with every part of the internal cavity of the chest, in opposition to the natural tendency of their structure, by the pressure of the atmosphere resting upon the internal surface of the air vessels through the windpipe. To whatever extent the atmospheric pressure, in dilating the lungs to the capacity of the thorax, is resisted by the elasticity of these organs, to the same extent is that pressure removed from the internal cavity of the chest. A balance of pressure will therefore rest against the external surface of the walls of the chest, equal in power to the resistance opposed by the elasticity of the lungs to the atmospheric pressure, in consequence of being kept in a forced state of distension. The walls of the chest will, therefore, be pressed in the direction of its centre by the whole amount of this balance. The base or floor is the only part of the thorax, which, in ordinary circumstances, will give way, in any considerable degree, to this force. The diaphragm must, in short, be always pressed upwards with a force equal to the elasticity of the lungs at the time; and, when affected by no other agents, will assume on the side facing the chest that convexity which this pressure against its inferior surface is fitted to give it.

In this explanation we discern a cause, interwoven with the structure of the lungs, rendered permanent and certainly efficacious, by the position and mechanism of the chest and its contents, clearly explanatory of all the phenomena, and, at all times, adequate to the accomplishment of that elevation which the diaphragm requires for the performance of its important functions.

When the diaphragm has been distended upwards, by the continued operation of the balance thrown, in the manner that has been explained, against its inferior surface to such an extent that the muscular fibres of which it is chiefly composed have been stretched beyond their natural length; these fibres, in consequence of the stimulus of pain; of volition to which this muscle is in a certain degree subservient; or of some other cause, exert a contractile effort; which, being more powerful than its antagonist, restores the diaphragm from the conical to the flat condition.

No sooner has the energy of the muscular action, agreeably to the nature of that action, been relaxed to a certain extent, than the antagonist but weaker power becomes in its turn

predominant, and, by the constancy of its operation, restores the cone formed by the diaphragm, to those dimensions from which it had been reduced, and, on its restoration to which, the dormant contractile power is again roused.

The contractile power of the diaphragm, in conformity with the laws of muscular motion, is irregular, remitting, and sometimes altogether quiescent. The elasticity of the lungs, on the other hand, is equal and constant. The superior energy of the former is balanced by the permanency of the latter. By the advantage which the inferior power, from the uniformity of its operations, is enabled to take of the remissions of its more powerful antagonist, the ground which had been lost is recovered, and the contest prolonged; that contest in which victory declaring on one side or the other is the instant death of the fabric.

If the question were asked, What constitutes the main spring of life? there could I think be no hesitation in replying that it consists in the contest which is maintained between the elasticity of the lungs and the irritability of the diaphragm, supposing the heart, as it certainly must be considered, an appendage of the latter.

The movements of the diaphragm and of the heart, though they proceed from similar causes, differ in some remarkable particulars. The actions of the heart are not subject to the control of the will. They are however evidently affected by the emotions of the mind. Whatever suddenly and powerfully arrests the attention has been found to suspend for a moment the actions of the heart. The circulation of the blood is very perceptibly affected by the passion of fear, of anger, or of joy. It has been not unreasonably supposed, that the will originally possessed a certain control even over the movements of the heart, but that it soon lost it in consequence of the disuse necessarily proceeding from the impracticable minuteness of attention, which the exercise of that control required. We may regulate at pleasure the quantity of air that is admitted into the lungs at any single inspiration, as well as the velocity with which it is admitted. The same may be observed with respect to our influence over the act of expiration; and we may shorten, and, to a certain degree, prolong the intervals between the termination and commencement of these alternating processes. But the control of the will does not extend to the interruption of respiration, at any stage beyond a

certain time, far less to the complete stoppage of it. Respiration proceeds at length in opposition to the most determined efforts of the will. It is performed with equal regularity and perfection, without the interference of the will; as during sleep, and at other times, when the attention is not directed to it.

During the process of expiration, the contractile influence of the diaphragm and other muscles employed in respiration is not altogether withdrawn of a sudden; it is only relaxed; and, by its gradual expenditure, ensures an easy progressive ascent to the diaphragm, and checks the resiliency of the ribs.

There are certain affections in which the contractile power of the muscles of respiration seems to be suddenly and completely suspended; as in the case of sneezing, spasmodic cough, and to a slight degree in hickup. In the first of these affections particularly, the opposition seems instantly to be removed from the resiliency of the lungs which collapse suddenly to a certain extent and with great force. In the case of hickup it is the contractile power chiefly which seems to have thrown off the controul of the will. The

motions of the diaphragm and intercostal muscles are evidently like those of the heart influenced by the passions of the mind.

It is chiefly to the motions of the diaphragm, that the respiration is to be ascribed; though in some animals, and especially in man, a considerable share of this office is to be attributed to the intercostal muscles. Those animals which are remarkable for swiftness and perseverance in the race, scarcely employ the intercostal muscles, but use the diaphragm almost solely in breathing.

The action of the abdominal muscles, though not necessary, may occasionally be useful in respiration. When any substance lodges in the bronchia which is too tenacious to be separated, or too ponderous to be elevated by the powers commonly employed in respiration, the contraction of the abdominal muscles will increase the pressure against the lower superficies of the diaphragm, and augment the velocity and the force with which the air passes through the bronchia and windpipe.

There may be also many diseases of the chest, in which the action of the abdominal muscles may be serviceable in respiration.

In no circumstances almost, during health, can it be supposed that the aid of the abdominal muscles is required to elevate the diaphragm; as the elasticity of the lungs is of itself sufficient for that purpose; and, unless it were counteracted by the residue of contractile energy subsisting in the muscular fibres of the diaphragm even during their relaxation, the cause might often be found too potent. In the act of sneezing, when the diaphragm, after a full contraction, is suddenly seized with a kind of paralysis, it is probable that the unresisting fibres are protected from the injury which they would be in danger of incurring, in consequence of the violence of the jerk to be sustained in their relaxed state from the inequality of pressure, by the synchronous and almost involuntary contraction of the muscles of the larynx and fauces. By this contraction the free exit of the air is resisted; the air in the chest confined and compressed, checks the collapse of the lungs, and of consequence the ascent of the diaphragm.

This defence however is not always effectual. Sneezing has been known to be suddenly fatal in consequence, as has been supposed. of the rupture of a blood vessel in the chest, but more probably of the rupture of the diaphragm itself.

The great object of respiration seems to be, to bring a quantity of atmospheric air, for a given time, in contact with the internal surfaces of the air vessels of the lungs. The most perfect respiration, therefore, would appear to be that, in which the extent of this surface differs most considerably between the lowest stage of expiration and the fullest inspiration. This object will be most perfectly accomplished by augmenting in inspiration the air vessels, both in diameter and in axis.

Those air vessels which run in a longitudinal direction, must be lengthened in axis, by the depression of the diaphragm. There are two ways in which the same vessels may be increased in diameter. The first is by increasing generally the diameter of the chest. As the contents of the blood vessels must be at all times nearly the same, and as the other parts are fixed and unchangeable, the widening of the chest at any section must be attended with a proportional increase in the calibre of the air vessels at that section. So far as the general diameter of the chest is augmented by the elevation of the ribs, in the same proportion must the diameter of those air vessels, which run from the apex to the base, be generally increased.

But there is another way in which the same effect is produced, and which is particularly necessary in those animals which breathe through the play of the diaphragm alone.

The chest forms a cone with its apex towards the neck, and its base facing the belly. As the lungs are at all times in contact with the internal cavity of the chest, and, as they are firmly attached to it at the apex, but at every other part free, the depression of the diaphragm must be accompanied with a lengthening of the lungs to the same extent; and this extension, it is reasonable to suppose, must be distributed equally through the whole length of the lungs; or every small portion of those organs must enjoy its own proportion of the increase. Any transverse section of the lungs, parallel to the base of the chest, must occupy a lower position in the chest, during full inspiration than it did during the lowest stage of the preceding expiration. The area of this transverse section will be larger, and, of course, the air vessels which run longitudinally, will be wider at that section during the former, than during the latter state, in consequence of the conical form of the chest. The air vessels, therefore, which run in a longitudinal direction,

will be widened as well as lengthened by the depression of the diaphragm; and those which run transversely will be lengthened as well as widened by the same cause.

Hence, we plainly discover the important purposes which are served by the shell of the chest being of a conical form.

As the causes, by which the diaphragm is elevated, are nearly the same with those which produce the diastole of the heart, and as the depression of the diaphragm, and the systole of the heart arise from the excited irritability of their muscular fibres, it is to be supposed that the two functions of respiration, and the circulation, will bear a strong relation to each other; and that, in many instances, whatever affects the former, will, in a similar manner, affect the latter.

The actions of the heart and diaphragm differ remarkably in one particular, which is the frequency of their repetition. Three pulsations are generally reckoned to take place in the time of a single respiration. The cause of this difference we shall not attempt to explain. It probably, in

some degree, proceeds from the difference in the dimensions of the two muscles, as in consequence of the longer range in the action of the diaphragm, a longer period for the performance of it is required. It may proceed also from the greater irritability of the heart.

It is evident, that neither the actions of the diaphragm, nor of the heart, could be performed, unless the former was elevated, and the latter dilated after contraction; and that, as the cause which produces those effects is nearly the same in both cases, whatever affects the operation of this cause, must be manifested by similar influences upon the functions to which these organs are subservient.

Accordingly, we find, from the numerous experiments that were made by Mr. Hunter, in particular, upon dogs, by making openings into the chest, that the free admission of the external air into this cavity, was accompanied with the collapse of the lungs, the depression or laxity of the diaphragm, and the diminution and often instant cessation of the actions of the heart. The result of these experiments is described by Mr. Hunter in the following words; in which,

according to his usual practice, he has mingled abundance of hypothetical reasoning with his more valuable observations.

"The diminution of the heart's motion," says he, "upon stopping respiration, does not depend upon the immediate impression of improper blood on the left auricle and ventricle, as a sedative, but upon the sympathetic connexion detween the heart and lungs; one action ceasing, the other also ceases; which sympathy is established, because, if the heart were to continue acting, it would send improper blood into the body, by which it can be supported only a little while."

In some of these experiments, the lungs were by an ingenious contrivance alternately expanded and allowed to collapse, and a quantity of fresh air passed through them, in imitation of natural respiration. By the renewal of that kind of respiration the actions of the heart were renewed; but, as the mechanical connexion between this organ and the lungs had been for ever dissolved, in a feeble degree only, and similar to that action which is observed to take place in the hearts of cold blooded animals immediately after abstraction from the body. "By throwing into the

lungs," says he, "fresh air, the heart began to act, upon which both auricles and ventricles became gradually smaller; then, by stopping respiration, they became larger and larger." The continued reapplication of fresh air to the lungs of these animals, delayed the departure of the principle of life from the heart, which was by that means put in the situation of the heart of a cold blooded animal newly abstracted from the body, and placed upon a warm stone or immersed in warm water.

The free admission of the external air into both sides of the chest must be attended with instant suffocation, and the annihilation of the actions of the heart. Nor can the latter of these conditions be supposed to follow, as a consequence of the former, but to be the synchronous and direct effect of the same cause.

The elasticity of the lungs may be too feeble, without any apparent organic affection. The want of due elasticity in the cartilages and membranes of the bronchia, may arise from an imperfection in original structure; may be hastened by circumstances in the progress of life; and will certainly occur from the frequency of use, and from

the imperfection inherent in all substances, in the course of old age. In all these cases, the actions of the heart will be feeble and often irregular.

Organic lesions of the bronchial vessels form another ample class of the causes, which impair the elasticity of the lungs. These vessels may be overstretched and extended to weakness, by violent respiration. This is, perhaps, frequent in those animals which are subjected to the tyranny of man, and is probably the general origin of that affection termed *broken wind*. The bronchial vessels may be corroded, softened into inelastic membrane, or hardened into bone, by disease. A laborious respiration will be the necessary effect of all these affections of the bronchia, and will, invariably, be attended by a languid irregular circulation.

The elasticity of the bronchia will necessarily be impeded, by the accumulation of extraneous substances in the lungs. Blood, water, or purulent matter effused into the air vessels, or cellular substance, would, in consequence of their specific gravity, of their incompressible nature, and of the difficulty with which they could be

removed, diminish the force and the range of the resiliency of the lungs; and would, upon the admission of the external air into the cavity of the chest, prevent these organs from collapsing with that vigour, and to that extent, which indicate their sound and perfect condition. In these cases, both the elevation of the diaphragm, and the dilatation of the chambers of the heart, would be weak and incomplete, and of course the functions of respiration, and of circulation, would be imperfectly performed. The same effect would arise from a surcharge of the blood vessels of the lungs, with their proper fluid. Any considerable tumors in the lungs, solid concretions, or conversions of substance, would, in like manner, lessen the force, and shorten the range of the elasticity of these organs, and would, invariably, be accompanied with a disordered respiration and circulation.

Another extensive class of causes, by which the respiration and circulation are equally affected, are foreign substances existing in the chest, but extraneous to the lungs themselves. These substances occupy a part of the space, which the lungs ought to fill, prevent their due distension, and reduce them at all times to a cer-

tain degree of collapse. They obstruct the dilatation of the lungs to that extent, at which their elasticity is the most powerful. Therefore, the elevation of the diaphragm, and dilatation of the heart, will be equally incomplete. Water, or any other fluid, contained between the cavity of the chest and the lungs, unnatural enlargements of any of the other viscera of the chest, tumors attached to these viscera, aneurisms of the large arteries, belong to this class of causes, by which the respiration, and the circulation of the blood, are both directly affected.

Though a very intimate mechanical connexion subsists between the organs employed in breathing, and those subservient to the circulation of the blood; these functions have their peculiar affections, and may, to a certain degree, be disordered independently of each other. The lungs may preserve their elasticity, while their capacity of being duly distended is impaired. Spasms affecting the lungs, tumors, or membranous excrescences, obstructing the entrance of the air into the trachea, or the passage through it, or any of its large ramifications, will impede the respiration, without directly aud mechanically affecting the movements of the heart. Paralysis, and or-

ganic derangement of the diaphragm, and great obesity or weight in the abdominal viscera, may be ranked among the same causes; to which, also, may be added ossifications of the cartilages of the ribs.

The disorders to which the circulation is subjected, unconnectedly with the respiration, are extremely numerous, in consequence of the mechanism of the heart being more complicated, and, of course, more liable to derangement. All organic affections of the heart itself, of the valves appertaining to it, of the blood vessels that issue from it, of the pericardium which invests it, will injure the circulation without directly and necessarily incommoding the respiration. Water, or other fluids, contained within the pericardium, may be included in this class of affections. The actions of the heart will be also more immediately deranged, by any error in the quantity, or in the quality, of the circulating fluid.

It is not to be understood, however, that the peculiar affections of the organs immediately employed in the circulation, can exist long, or to any considerable extent, without being, in some form, communicated to the organs of respiration or

that any considerable disorder on these last will not soon be marked by some irregularity, or imperfection in the circulating system. These two functions are of such vital importance, that any considerable derangement in the one, will not only be soon felt by the other, but will extend itself to the most remote and least considerable parts of the animal machine.

The above imperfect classification is meant as an attempt, (a feeble one indeed) to distinguish those affections of respiration, and of the circulation, which are original and immediate in both functions, from those which originally belong to the one, and which are only consequential and derivative in the other, and is principally intended as an illustration of the manner in which they are supposed to be connected.

If the principles from which the connexion between these functions has been traced, be founded in truth, the further prosecution of this line of investigation may lead to a more distinct and less fallible diagnosis of the seats of the diseases affecting the organs of the chest.

As the condition of the organs both of respiration and of those principally concerned in

the circulation of the blood is in many respects different before birth, from that in which it is afterwards, it is proposed to conclude this appendix with a brief examination of these organs as they exist in the fœtus.

The lungs of the child before birth are at all times in a state of complete collapse; their specefic gravity is greater than that of water; the bronchia and air cells contain no air; and the dimension which these organs fill, are less than those into which they can be compressed after a single inspiration by any force which does not destroy their fabric.

The part of the chest which is after birth, occupied by the expanded lungs, is before that event filled by the thymus gland, which is very large in the fœtus, by the heart, and principally by the greater dimensions of the cone formed by the diaphragm, which swells up with a high convexity into the thorax. The liver which is also very large in the fœtus and other abdominal viscera, following the ascent of the diaphragm, occupy a higher place in the trunk of the body before than after the commencement of respiration.

In attempting to ascertain the causes of those changes which take a place in the organs of respiration immediately after birth, we shall begin with inquiring into the condition of the diaphragm before that event.

The hypotheses which have been advanced respecting the causes of the first inspiration are so numerous that the mere statement of them would occupy far greater room than can in this appendix by assigned for such a purpose. Suffice it to say, that they have generally proceeded upon the supposition, that the diaphragm is at all times in a quiescent inactive state during the residence of the child in the womb, and that it is not till after the period of birth that its contractile power is exerted.

But there does not appear to be at all any good reason for this assumption. The muscular fibres are as perfect before as they are after birth. From the vigorous motions which the child is known to make long before birth, it is evident, that it possesses the control generally over those muscles which are afterwards known to be subject to the will of the newly born infant. With the exception of that painful feeling, which is

derived from the suspension of breathing, which is unknown to the fœtus, and which is only occasionally experienced by the adult, the causes, (particularly that very powerful one springing from the distension of the fibres) by which the muscular fibres of the diaphragm are afterwards excited, exist at least in as great an extent before as after birth. It may therefore be presumed that it is not for the want of effort on the part of the diaphragm, that it does not descend before birth, but from the impossibility of the descent being effected in the circumstances in which that organ is then placed, by the ordinary efforts.

It is sufficiently plain that the diaphragm could not be flattened by any contractile effort of its muscular fibres, unless some fresh substance was at the same time received into the boundaries of the chest; to occupy the space which this change in the form of the diaphragm would leave, and to preserve the equilibrium of pressure between the substances within and those without this cavity. The principle passage, through which such an addition could be made to the contents of the chest, and the only one, by which the bulk of the lungs could be affected, is the windpipe. With respect to the large veins through which blood

situate without the confines of the chest might in such circumstances enter these confines, there will afterwards be an opportunity of speaking.

The following causes seem to prevent the passage of any substance through the windpipe into the lungs during the residence of the fœtus in the womb, notwithstanding the natural tendency which the substances surrounding it would have to take that course in consequence of the supposed contractile effort of the diaphragm.

The first, in all probability the most effectual, and perhaps the only cause required, is the unfitness of the fluid in which the fœtus is immersed for entering the larynx and descending through the trachea. Every one knows from painful experience the offence which is given to the muscles of the larynx, and to those which are employed in respiration generally, by the entrance attempted to be made into the windpipe by any other fluid except air; how carefully the approach of such a foreign hostile invasion is guarded, and the strong and united efforts which, upon the event of a surprise, are instantly made to repel the ungrateful intrusion.

In cases of immersion under water when the painful and anxious sensations, which the absence of that quality derived in respiration from the air produces, and from which the fœtus in utero is free, excite the most strenuous exertions to dilate the chest, the guards against the admission of water into the trachea are so well maintained, that even in the event of drowning, a small portion of this fluid only is found to have entered the chest, and that has descended it is supposed only after the irritability of the muscles of the larynx has been impaired or annihilated. The guards against the intrusion of offensive substances into the trachea may be allowed to be able to perform their office more effectually in the fœtus which is not impelled by such painful sensations to overcome them.

Other circumstances may possibly concur to protect the trachea of the fœtus from the entrance of the liquor amnii. One of these may be the pendant position of the head upon the top of the chest, and which, in consequence of its weight, the struggles of the child cannot alter.—This, along with other circumstances connected with the state of the fœtus in utero and affecting the passages into the trachea, may by compres-

sion or after the manner of valves, mechanically close the passage into the lungs. The existence however of these causes of protection is in a great measure hypothetical, and in all probability may not be required.

The position of the knees and the pressure of the abdominal viscera could have little effect in resisting the descent of the diaphragm, provided the passages into the lungs were open to the admission of the surrounding fluid.

As soon however, as the child is exposed to the air, the grateful sensations which the application of this fluid is naturally fitted to make upon the muscles of the larynx are experienced; the guards are withdrawn; a free passage is opened to the admission of the grateful visitant; the diaphragm repeats, and for the first time with success, its efforts to be relieved from the stimulus of distension; and the lungs are dilated.

The causes, which render necessary the repetition of the process of respiration after it has been once performed, have been already fully explained.

The lungs of the unborn fœtus, as they are in a state of complete collapse, can have no resiliency, and, therefore, no influence in producing the expansion of the chambers of the heart after contraction, in the manner supposed in the adult. But the heart beats and the blood circulates in the fœtus in utero as well as after birth. Here then is a case in which the heart must be dilated after contraction, without the assistance of that cause to which this effect has been chiefly ascribed in the preceding inquiry. And, if the effect can be produced without the supposed cause in one instance, it may be urged that it is unnecessary and unreasonable to have recourse to it in any other.

With a view to the explanation of the manner in which the blood is circulated in the fœtus and to the removal of the objection which has been stated, the few following observations seem necessary.

No observations, according to my recollection, have been recorded, by which the force and velocity of the circulation in the unborn fœtus have been ascertained. As some of the most important offices which the circulation serves

after birth are otherwise supplied in the fœtus, the same vigorous circulation would not appear in that situation to be necessary. The equal distribution of heat through the body and the preservation of it in the same temperature in different climates, offices requiring the aid of the circulation after birth, are before it indebted for the requisite aid to that of the mother. The frequent return of the blood to the lungs for renovation, so indispensible after birth, is for the same reason unnecessary in the fœtus. The chief office, which the motion of the blood serves before birth, would appear to be the conveyance of nourishment to the different parts of the frame; and, to that purpose a languid circulation would seem to be better adapted than a motion of greater rapidity.

On reference to the part of the preceeding inquiry in which the causes of the expansion of the chambers of the heart are detailed, it will be found that the walls of the ventricles of the heart derive the property of dilating themselves after contraction, to a certain degree, from their natural structure. In consequence of the circular direction of the muscular fibres it was there contended that the dilatation of the chambers was

the necessary effect of the simple relaxation of these fibres. This inference from the structure of the heart appeared to be confirmed by observation on the hearts of cold blooded animals, newly abstracted from the body.

But upon considering the situation of the contents of the chest and of the diaphragm of the fœtus, another cause will in this case be found for the dilatation of the heart after contraction. The diaphragm of the fœtus is not pushed up into the chest, as is generally supposed, but is drawn or held up to occupy a part of the space which is afterwards filled by the dilated lungs. This so different from that which it assumes after birth, may easily be supposed to be a forced position of the diaphragm. The elastic substance which unquestionably enters into its composition must be at all times, to a certain extent, upon the stretch. The heart in consequence of the resiliency of the diaphragm and the situation into which it is forced in the fœtus, must be drawn downwards from the lungs. The diminution of pressure arising among the contents of the chest from the tendency which the diaphragm has to separate from the lungs, will necessarily, upon the fibres of the heart becoming relaxed

and its boundaries dilatable, cause an influx of blood from the veins into the organ, and the dilatation of its chambers, in the same manner, though perhaps not with the same force, as that with which the diastole of the heart has been supposed to be effected after birth by the resiliency of the lungs.

If, however, the elastic power which is supposed to reside in the diaphragm, and to be called into action by the situation in which that organ is placed in the fœtus, should be disputed, there is a power in reserve derived from the irritability of the diaphragm, respecting the existence and efficacy of which there can be no doubt. It is probable that the powers, which the infant possesses over the muscles subject to the will, are connate; and that its influence over the movements of the diaphragm is as early as that which it has over the muscles by which the limbs are extended or bent; but it is also probable that this influence, with respect to the diaphragm, is rendered ineffectual during the residence of the fœtus in the womb, by the circumstances in which that organ is there placed. The diaphragm may, however, even in the fœtus be supposed to contract and to descend to a cer-

tain extent, and the dimensions of the cone formed by the diaphragm will be in an inverse ratio to the bulk of the heart during its diastole and systole; and, as, in consequence of the communications which subsist between the different chambers, the heart of the fœtus may be considered as a single reservoir, and therefore nearly emptied during contraction; the general dimensions of the heart may vary more before, than they are allowed to do after birth. The muscles which are after birth subservient to respiration, may even before that event, be supposed to enjoy a certain play, by which blood would be pumped into the heart from the veins, in the same manner in which the air is afterwards drawn into the lungs from the atmosphere, by the act of inspiration. If this be the sense in which some authors have used the term respiration, when they contend that this process is performed to a certain extent before birth, the opinion is not to be despised, and is by no means deserving of that ridicule with which it has been treated by some late authors.

Whether the diaphragm be supposed to descend in any degree in the fœtus or not, is a matter of no importance in the present discussion;

all that is contended for, is, the effort to produce a descent, either in consequence of elasticity, or of excited irritability. And if the effort should not be followed by any movement, that will happen because one part of the heart swells and is filled as the other is contracted and diminished; and because this organ, as has been contended to be the case after birth, is, also before it, at all times of the same dimensions.

FINIS.

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